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ORIGINAL ARTICLES.

THE PHYSIOLOGICAL ACTION OF COCAINE ON THE COMMON FROG, WITH SPECIAL REFERENCE TO ITS ACTION ON ORGANS AND TISSUES.[1]

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In the following experiments the hydrochlorate of cocaine, prepared by Merck, of Darmstadt, was used. It is uncrystallized, of a dull white color, has a slightly bitter taste, nearly neutral reaction, and is very soluble in water, alcohol, ether and chloroform. The solutions used were all made with water, and were 1, 2 and 4 per cent. in strength.

Local Action.—On the skin and mucous membrane: If the foot of a frog be immersed for a few minutes in a solution of cocaine (2 per cent.), very marked anæsthesia of the skin is produced; or, if a solution (2 or 4 per cent.) is applied to the mucous membrane of the mouth, the same action is observed.

On the eye: When a few drops of a 4 per cent. solution are instilled into the eye of a frog, there is caused, after a short time, almost complete anæsthesia of the conjunctiva and cornea. The action on the pupil is not constant, but usually a moderate degree of mydriasis is produced.

General Action.—The effects of cocaine given by hypodermic injection begin to manifest themselves very quickly, and in a few minutes have reached their maximum. When large doses are given, the action is seen almost immediately, and the duration and severity of the symptoms are in proportion to the amount used. After small doses the animals begin to recover at the end of a few minutes; but if the quantity administered is large, it is many hours, and it may be several days, before the effects have entirely disappeared. Lethal doses are from .04 grm. upwards and when death occurs from the effects of cocaine, it is only after the lapse of 24 or 36 hours.

Small Doses.—When a small toxical dose of cocaine (.005 grm.) is injected under the skin of a frog there are usually at first no perceptible effects. In some cases there is a short period of excitement, but this is not always present, nor is it as a rule, very strongly marked when present. The frog soon becomes quiet, he moves less quickly after stimulation is applied, and his respirations are somewhat increased in number. In a few minutes he becomes perfectly motionless, and changes his position only after considerable irritation, and then with an evident effort. Presently his head drops, the respirations become slower and the pupils somewhat dilated. Twenty or twenty-five minutes after receiving the poison he lies flat on his abdomen and neck, with the posterior extremities extended, and is unable to raise or balance himself. If strong stimulation is applied, he makes a vain effort to spring, which sometimes takes the form of a partial clonic convulsion. There is marked loss of power over his extremities, and this appears earlier and is much greater in the posterior than in the anterior. The action of the heart is somewhat slower than normal. In a short time he begins to recover, his respirations are more frequent and deeper, he raises his head and is able to balance himself when not disturbed. The last function to return is complete voluntary power in his limbs. An hour or an hour and a half after receiving the poison, the animal has become quite normal again.

Moderate Doses.—After moderate toxical doses (.01 grm.) given hypodermically, as after small doses, there is sometimes a short period of excitement; but this, even when present, soon gives way to the depressing action of the drug. The animal becomes perfectly quiet, the respirations slow and superficial, and the heart's action diminished in both force and frequency. At the end of seven or eight minutes the frog can no longer support himself and lies extended at full length. His toes being pinched he makes two or three powerless efforts to move and then remains quiet. The loss of power and also of sensation appears earlier, and is more marked in the hind legs than in the front legs, as in the first experiment. The pupils are somewhat dilated. After about twenty minutes, he becomes perfectly relaxed and remains in any position in which he is placed. His respirations are imperceptible and only weak reflex movements occur after severe mechanical stimulation, although a moderately strong interrupted current calls forth muscular contractions.

An hour later the animal lies in the same position. Neither the action of the heart nor any respiratory movement is perceptible, and the reflexes are less marked than before. About two hours and a half after receiving the poison, the frog begins to show signs of recovery. It makes some slight voluntary movements and the reflexes become stronger. Three

[1] These experiments were made in the physiological laboratories of Profs. Dubois Reymond and Christian, in Berlin.

hours later he turns over on his belly, and irritation calls forth characteristic clonic contractions. There are as yet no signs of returning respiration. In twelve or fourteen hours the frog has entirely recovered.

Large Doses.—When large toxical doses of cocaine (.025 grm.) are given to a frog by hypodermic injection, the effects begin to manifest themselves almost instantly. All the phenomena observed in the previous experiments, here follow each other in rapid succession and in a much more intense form. There is no period of excitement. In two or three minutes respiration is labored, pupils dilated with opisthotonus, heart's action slow and reflexes greatly diminished. The frog has a very anxious expression and in a few minutes more he drops motionless on the table, extended at full length. There is only a weak reflex after severe irritation. Respiration ceases with the lungs in inspiration. After many hours, if the dose has not been lethal, signs of returning life begin to show themselves, but it is two or three days before the animal has entirely recovered. After the exhibition of a lethal dose, the heart continues to act for twenty-four or thirty-six hours and then stops in diastole.

Action on the Heart.—When a frog has been poisoned by cocaine by hypodermic injection, the action of the heart is either unaffected or there is a diminution in the frequency of its pulsations. After very small doses (.001 grm.) there is no perceptible influence. When a somewhat larger amount is given (.005 grm.), the frequency of the heart's action is reduced from eight to twelve pulsations a minute, and it becomes irregular and intermittent. In the course of two or three hours the influence of the drug has passed off and the action of the heart becomes perfectly normal again. Large toxical doses (.025 grm.) cause an almost immediate reduction in the number of pulsations to six or eight per minute. The heart becomes engorged with blood and its action is very much labored and irregular. The alternate action of the auricles and ventricle is replaced by an irregular action, and long pauses occur in the pulsation of the ventricle. The ventricle is affected earlier and more severely than the auricles, and when paralysis finally occurs, if a lethal dose has been given, the ventricle ceases to pulsate a long time before the auricles.

Experiment I.—Medulla destroyed. Heart exposed. ♏ iv. sol. cocaine (1 per cent.)=.0025 grm. injected hypodermically.

Time.	*Minutes after Cocaine given.*	*Pulsations of Ventricle in 15 sec.*	*Pulsations of Auricles in 15 sec.*	*Amt. Cocaine.*	*Remarks.*
11.20	.....	12.12	12.12	grm.	
11.24	.....	12.12	11.11	.0025	
11.26	2	11.11	11.11	.....	
11.30	6	11.11	11.11	.....	Action somewhat labored.
11.40	16	10.11	10.11	.....	
11.45	21	10.11	10.11	.....	
11.55	31	10.11	10.11	.....	
12.05	41	11.11	11.11	.....	

Experiment II.—Medulla destroyed; heart exposed. ♏ viij. sol. cocaine (2 per cent.)=.01 grm. injected hypodermically.

Time.	*Minutes after Cocaine given.*	*Pulsations of Ventricle in 15 sec.*	*Pulsations of Auricles in 15 sec.*	*Amt. Cocaine.*	*Remarks.*
3.43	.....	11.12	11.12	.....	
3.45	.....	12.12	12–12	.01grm	
3.47	2	11.11	11.11	.....	
3.57	12	8.8	8.8	.....	Irregular. Occasional long pauses.
4.01	16	8.8	8.8	.....	
4.04	19	8.9	8.9	.....	
4.10	25	7.7	7.7	.....	Irregular and intermittent. Occasional pauses of ventricle while auricles continue to act.
4.15	35	7.7	8.9	.....	
4.30	50	7.8	8.9	.....	
4.36	56	7.8	10.9	.....	
4.52	1h 12m	8.9	9.8	.....	
5.00	1h 20m	9.9	9.9	.....	Acting regularly.

Experiment III.—♏ x sol. cocaine (4 per cent.) .025 grm. injected hypodermically.

Time.	*Minutes after Cocaine given.*	*Pulsations of Ventricle in 15 sec.*	*Pulsations of Auricles in 15 sec.*	*Amt. Cocaine.*	*Remarks.*
4.12	.....	11.12″	11.12″	grm.	
4.14	.....	11.12	11.12	.025	
4.17	3	4.3.4	8.9	.....	Very irregular and labored in its action.
4.21	7	.....	7.8	.....	
4.24	10	3.3.3	6.6	.....	Heart engorged with blood. Ventricle cannot completely empty itself.
4.27	13	3.2.3	4.5	.....	
4.33	19	3.7	6.6	.....	Action very irregular and intermittent.
4.35	21	3.2.0	6.6	.....	Pause of 15 sec. in action of ventricle.
4.40	26	0.0.0	4.4	.....	Pause of 60 sec. in action of ventricle.
4.50	36	3.2.0	5.5	.....	
5.00	46	0.0.3	5.6	.....	

If the heart of a frog be exposed and three or four minims of a (4 per cent.) solution of cocaine be injected directly into the ventricle, the heart is immediately paralyzed and stops in systole. If now, as a contra experiment, into the heart of another frog the same amount of water or normal salt solution be injected, no effect is produced save that arising from the physical injury, and this soon passes away; but afterwards the injection of this same quantity of a (4 per cent.) solution of cocaine causes, as before, immediate paralysis of the heart in systole.

Again, when three or four minims of the same solution of cocaine are injected into the median abdominal vein, or into one of the great veins of the heart, paralysis either immediate or after two or three beats results. In these cases the heart stops in diastole.

The local application by means of a camel's hair pencil of a weak solution (1 per cent.) of cocaine to the heart muscle causes a temporary diminution in the frequency of its pulsations.

Experiment IV.—Medulla destroyed. Exposed heart.

Time.	*Pulsations of heart in 1 minute.*	*Amount Cocaine.*	*Remarks.*
10.25	33	.....	
10.50	34	.....	
11.	34	Cocaine in 1 per cent. solution.	
11.04	24		Heart somewhat distended with blood.
11.13	29	..	
11.18	24		
11.34	34	..	
11.36	17		Action labored and irregular, but soon
11.44	30	..	becomes normal in frequency and character.
11.47	17		Irregular. Heart engorged with blood.
12.10	26		

Action of Cocaine in Substance on the Heart.—Experiment V.—Medulla destroyed; heart exposed. Cocaine in substance applied to both auricles and ventricles. In about three minutes the heart had ceased to pulsate. The ventricle is in systole, and the auricles are in diastole.

Experiment VI.—Medulla destroyed; heart exposed. Cocaine in substance applied to heart. After four minutes the heart had ceased to pulsate. Ventricle is in systole, and the auricles in diastole.

Four or five hours later, in neither of these cases had the heart commenced again to act. (See Experiment VII.)

Experiment VII. — Conditions same as previous cases. Results same. The ventricle is firmly contracted and pale. The auricles large and distended with venous blood. In the last case, eighteen hours later, the heart was found to be again pulsating slowly and feebly. It continued to act for about six hours, and then stopped. In a number of succeeding experiments it was observed that in most cases, after many hours, the heart commenced again to act, but this was not true in all cases, and when the solution was injected into the cavity it never occurred. In all cases the ventricle was in systole, and in all but one the auricles were in diastole.

Cocaine in substance applied to either one of the auricles, or to the ventricle separately, caused a temporary paralysis of the respective portions of the heart, and a diminution of the frequency of the pulsations of the heart itself, but not paralysis, unless it was used in large quantity.

Influence on Respiration.—Small toxical doses of cocaine (.0025 grm.) cause at first a slight increase in the frequency of the respirations, but soon they become much diminished, and are reduced to six or eight per minute. These are very superficial, and sometimes, even with these small doses, all respiratory movement disappears for a short time.

After medium-sized doses (from .005 grm. to .01 grm.), the temporary rise in the number of respirations per minute is less marked, and the depressing effects of the poison are more quickly observed. The decrease in the frequency is much more rapid than after small doses, and respiration usually becomes imperceptible in about twenty-five or thirty minutes.

With large toxical doses (from .025 grm. upwards) there is no semblance of a temporary increase. The respirations rapidly diminish in number and change in character, and usually in ten minutes have entirely disappeared. After these large doses respiration stops after a full inspiration; in the other cases apparently after expiration. Respiration does not appear again until after at least twenty-four hours, and often it is a much longer time.

Experiment VIII.—♏ iv. sol. cocaine (1 per cent.) = .0025 grm., injected hypodermically. Frog free under glass jar.

Time.	*Minutes after Cocaine given.*	*Respirations in 15 sec.*	*Amt. Cocaine.*	*Remarks.*
10.15		18	.0025	
10.17	2	20	.....	
10.20	5	24	.....	Irregular but rather deep.
10.22	7	20	.....	
10.25	10	14	.....	Quick, jerky and superficial.
10.28	13	12	.....	
10 30	15	10	.....	
10.33	18	7	.....	
10.36	21	5	.....	Long.
10.38	23	8	.....	Very superficial.
10.40	25	16	.....	Very superficial; scarcely perceptible.
10.44	29	12	.....	
11.12	57	18	.....	Natural.
11.20	1h. 5m.		.....	

Experiment IX. — ♏ viij. sol. cocaine (2 per cent.)=.01 grm. injected hypodermically. Frog free under glass jar.

Time.	*Minutes after Cocaine given.*	*Respirations in 15 sec.*	*Amt. Cocaine.*	*Remarks.*
10.48	.....	18	grm.	
10.50	.....	18	.01	
10.53	3	22	.....	
10.55	5	20	.....	
10.57	7	18	.....	
11.03	13	16	.....	
11.07	17	9	.....	Very superficial.
11.15	25	8	.....	Scarcely perceptible.
11.28	38	None	.....	

Experiment X.—♏ x. sol. cocaine (4 per cent.)= .025 grm. injected hypodermically. Frog free under glass jar.

Time.	*Minutes after Cocaine given.*	*Respirations in 15 sec.*	*Amt. Cocaine.*	*Remarks.*
11.33	.....	18	grm.	
11.35	.....	18	.025	
11.37	2	16	.....	Difficult respiration.
11.38	3	14	.....	
11.39	4	12	.....	Very superficial.
11.40	5	None	.....	

ACTION ON NERVOUS SYSTEM.

Spinal Cord.—Very small doses of cocaine slightly increase the reflex irritability of the spinal cord for a short time. When larger doses are given, the period of heightened reflexes is very short (if it is at all present) and then follows a period of much longer duration, when they are decidedly diminished. After large toxical doses, there is no period of greater irritability; but almost immediately the general reflexes are very much lessened, and there is soon almost complete loss of all response to mechanical or chemical stimulation. Loss of response to strong electrical stimulation never occurs except just before death.

Sensory and Motor Nerves.—(From B. von Anrep). Small doses heighten the irritability of the sensory fibres without having any effect on the motor filaments. Large doses cause after a slightly increased irritability of the sensory fibres of short duration a diminution of the same. Large toxical doses almost completely paralyze the sensory filament, and diminish greatly the irritability of the motor. Tactile reflexes are much diminished. The pneumogastrics are completely paralyzed after doses of moderate size. The small doses have no perceptible influence on the inhibitory fibres of the pneumogastrics. After the poisoning they either remain as before responding to the same strength of current, or they are wholly paralyzed.

Experiment XI.—Heart exposed. Both pneumogastrics divided.

Time.	*Contract's of the Ventricle in 15 sec*	*Contract's of the Auricles in 15 sec.*	*Amt. Cocaine.*	*Remarks.*
8.30	13.14.14	13.14.14	.....	
8.40	14.14.14	14.14.14	.....	Stimulation of rgt. pneumogastric nerve.
.....	7.4.2	7.4.2	.....	
8.55	13.14.14	13.14.14	.. ..	Stimulation of left pneumogastric nerve.
.....	3.3.1	3.3.1	.....	
9.05	14.14.14	14.14 14	.03grm	
9.10	7.6.6	9.6.6	.....	Stimulation of right P. nerve.
.....	5.5.5	8.8.8	.....	Stimulation of left P.
9.20	5.4.5	8 8.8	.....	
10.00	2 2.2	4.5.4	.....	
11.00	2.1.2	3.4.3	.....	

Local Action on Nerve-Trunks.—Cocaine in weak solution (1 per cent.). When applied directly to the nerve-trunks after a very transient period of increased irritability, depresses greatly and finally paralyzes the nerve. If it is applied in strong solution (4 per cent. or more), the paralyzing action is almost instantaneous, and is attended by no period of increased irritability. If, however, the solution is not left in contact with the nerve for too long a time (and the nerve is still in its natural connection), it will regain all its former irritability in from one and a half to two hours.

Experiment XII.—A preparation of the sciatic nerve and the gastrocnemius muscle of a frog is made in the usual manner, and in as fresh a state as possible, is arranged on the muscle-telegraph of Prof. Dubois Reymond, and is then connected with the induction coil of his Sliding Induction apparatus, a simple key intervening. The nerve is placed in a moist chamber and non-polarizable electrodes are used. A 1 per cent. solution of cocaine is applied by means of a camel's-hair pencil to the nerve from time to time. The figures in the second column indicate in centimetres the distance that the secondary coil is from the primary at the time that contraction occurs.

Time.	*Centimetres.*	*Amt. Cocaine.*	*Remarks.*
10.10	70	1%	
10.20	43		
10.25	37	1%	
10.30	33		Contractions very feeble. Cannot produce tetanus.
10.35	No response.		

Control.—Experiment XIII.—Conditions same as last. Water used first, then solution of cocaine.

Time.	*Centimetres.*	*Water and Sol. Cocaine.*	*Remarks.*
10.15	93	Water.	
10.20	97*	.....	97 is limit of the distance of second coil from first.
10.30	97	. ....	
10.40	97	.....	
10.45	97	Cocaine. in 1% sol.	Active contractions.
11.15	No response.		The irritability rapidly diminished after the application of a solution of cocaine, and at the end of 30 minutes the nerve was completely paralyzed.

Experiment XIV.—Conditions same as XIII.

Time.	*Centimetres.*	*Water or sol. Cocaine.*	*Remarks.*
11.10	87	Water.	
11.20	97	.....	
11.40	97	.....	Very active contractions.
11.50	46	.....	
12.05	42	.....	
12.35	36	Cocaine 1% sol.	After being in water for one hour and a half, all irritability disappeared in six minutes when cocaine applied.
12.41	No response.		

*The irritability of a nerve is often greater a short time after removal than at time of removal.

Experiment XV.—Conditions same as XIII. Cocaine in 4 per cent. sol. used.

Time.	*Centimetres.*	*Sol. Cocaine 5%.*	*Remarks.*
10.50	50	.....	
10.55	50	4%	
10.56	47	.....	
10.59	No response.	.....	In a number of other experiments with the action of a 1% sol. on nerves, all irritability had disappeared in from 15 to 35 minutes.

Experiment XVI.—If a nerve muscle preparation be made from a frog poisoned by cocaine, the irritability of the nerve is found to be much diminished. (This is also true of the muscle.) The strength of current required to produce a muscular contraction is decidedly greater than for that from a normal frog. The muscular pulsations are shorter and less powerful, and it is impossible to produce tetanic contractions (if the amount of the drug given has been at all large). When the interrupted current is passed through the nerve for some time, there are several short, quick, feeble pulsations of the muscle caused and then there is no more response. The contractions are very similar to those produced by stimulation applied to a nerve poisoned by the local application of a solution of cocaine.

Experiment XVII.—Medulla destroyed. Sciatic nerve of one leg carefully exposed, as little injury being done to the vessels as possible. The irritability of the nerve is accurately determined with the sliding induction apparatus. A solution of cocaine (1 per cent.) is then applied till the nerve only responds to the stimulus of a very strong current. Normal salt solution is then substituted for the solution of cocaine, and in the course of an hour and a half the irritability of nerve has completely (or almost completely) returned.

Experiment XVIII.—Medulla destroyed. Sciatic nerve exposed on one side and a cord is passed carefully under it and around the limb so as to include all the tissues of the limb except the nerve, and is then firmly tied—the frog is poisoned by a moderate dose of cocaine. The nerve of the other leg is now exposed, and the comparative irritability of the nerves on the two sides is determined. It is found that it requires a much stronger current to produce a muscular contraction in the leg not shut off from the circulation than in the bound leg. If the irritability of the nerves is tried from time to time, it is found that the irritability gradually returns to the poisoned nerve, and that in the course of an hour and a half or two hours, it has become as great as that of the protected leg. From these two experiments it is seen that the irritability of nerve-trunks, whether diminished by the local application of a solution of cocaine or by cocaine given by hypodermic injection, is regained in about the same length of time. It would therefore seem probable that the action in both cases was a purely local one.

Experiment XIX.—If the nerve from a nerve muscle preparation be immersed for a few seconds in a solution of cocaine and then be removed, it is found that the irritability is very much increased. The slightest touch of a steel instrument throws the muscle into violent tetanic contractions. If the nerve be now again immersed for a longer time, the irritability becomes very much diminished and is finally lost to all forms of stimulation. When the nerve is first placed in the solution, no irritation is caused by the immersion.

The influence of cocaine on the nervous system would not be complete without an allusion to the apparently marked effects of the drug on the power of coördination, which all frogs poisoned by cocaine show. It is particularly manifest after doses of moderate size.

Action on Striated Muscles.—Small doses of cocaine given hypodermically have little effect on the excitability of the voluntary muscles, but large doses diminish their excitability very decidedly. The character of the muscular contraction called forth by electric stimulation is greatly altered. The pulsations are shorter, slower, and more feeble, and the muscular excitability is soon exhausted. The local action of cocaine on muscular tissue is very similar to that found in general poisoning.

Experiment XX.—Gastrocnemius of frog without the nerve is prepared in the usual manner, and is arranged on the muscle-telegraph, and is then connected with the induction coil of the sliding induction apparatus. The numbers in the second column represent the number of centimetres that the induction coil was distant from the primary coil when contraction occurred. Cocaine in 1 per cent. solution was applied to the muscle by means of a camel's-hair pencil.

Time.	*Centimetres.*	*Amt. Cocaine.*	*Remarks.*
12.03	24	1%	
12.05	23	.....	
12.07	18	.....	
12.10	17	.....	
12.11	17	1%	
12.18	16	.....	
12.22	16	1%	Responds feebly, and after two or three contractions responds no more.
12.26	15	.....	
12.30	13	.....	Very feeble response to very strong current.

The muscle retains the slight excitability, remaining at about this point for some time.

Experiment XXI.—Preparation and arrangement same as in Ex. xx. sol. cocaine 2 per cent. used.

Time.	*Centimetres.*	*Cocaine applied.*	*Remarks.*
12.35	26	.....	
"	26	2 per cent.	
12.38	20	.....	
12.40	18	.....	Quick, short, pulsations.
12.44	.....	2 per cent.	
12.50	16	2 per cent.	
12.55	15	.....	Contractions slow and feeble.
1.07	12	.....	Responds very feebly.
1.15	5	.....	Scarcely perceptible contractions to very strong current.
1.25	No response.	.....	

The application of cocaine in substance to muscle produces a gradually decreasing excitability until after twenty or twenty-five minutes there is complete loss of all excitability and there is no response to the strongest interrupted current. It requires many hours for a muscle to regain its excitability when it is to any considerable extent destroyed by either the local or the constitutional action of cocaine. Here again the local and the general actions of the drug are very similar.

In conclusion, then, the action of cocaine on the frog may be summed up as follows:

1. It has a powerful local anaesthetic action on the skin, mucous membrane and the eye. It usually produces mydriasis.

2. It has a depressant action on the heart, reduces the force and frequency of its pulsations and finally paralyzes it (first the ventricle and then the auricles) in diastole.

3. In small doses it at first slightly increases the number of the respirations, then decreases them, and in large doses diminishes them rapidly from the first, finally causing death from a paralysis of respiration.

4. It at first slightly heightens, and then greatly depresses the reflex action of the spinal cord in small doses. Large doses depress from the first.

5. Small doses at first slightly increase the irritability of the sensory nerves, then depress their irritability, and large doses depress from the first.

6. Both large and small (not very small) doses have a depressant action on the motor nerves.

7. It paralyzes the pneumogastric nerves.

8. Doses of moderate size diminish the excitability of the striated muscles.

9. The local application of cocaine to any of the more highly constituted organs or tissues causes a temporary cessation of their functional activity.

10. From the local and constitutional action on the different organs and tissues, it is rendered probable that its general action is wholly a local one, exercised on all parts for which it has a chemical affinity, through its presence in the blood.

The results of these experiments would seem to indicate the use of cocaine in tetanus and strychnia poisoning.

The writer hopes to be able to report in a short time the results of similar experiments on the warm blooded animals, with experiments on the antidotal action of strychnia and cocaine.

FRAGMENTARY COMMENTS ON SOME OF THE PRINCIPAL METHODS OF OPERATION FOR "STONE IN THE BLADDER."

With Special Reference to Allarton's Method, Also in Combination with Lithotripsy, Urethro-Lithotripsy, Urethrotomia Lithotriptica, Including Besides Critical Remarks Upon the Discussion of the Progress Made in Lithotripsy and "Lithotomy" at the Late International Medical Congress held in London, England, and divers other subjects comprising the Urinary Organs.

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[CONTINUED.]

The Lateral and Bilateral Cystotomy (*"La Taille" of the French*).—Some authors of to-day recognize only two main methods of *cystotomy*, 1, *the inferior C*, where the membranous and prostatic portion of the urethra, with the neck of the bladder, are cut, and of which they cause two subdivisions, or, *the lateral*, and, *the bilateral C;* and, 2, *the superior C*, whilst the pærineal median and others are simply mentioned. Of all the different methods of cystotomy the lateral section is preferred by most surgeons, and most frequently performed since its introduction into England by Cheselden. So far it has also given the best results in children previous to puberty. The best advice in performing the operation is to keep the point of the knife high up, on account of the high position of the bladder, which, also, by being partially filled with warm water, will descend lower.

Concerning the *bilateral operation* we find that the authors are of different opinions. With some the body of the prostate and the membranaceous part of the urethra have to be divided, while others want the neck of the bladder included, as *Le Dran* and *Senn. Vidal de Cassis* wanted even the neck incised in four different opposite directions, to accomplish which *Colombat* constructed a separate curious knife, which he called "*lithotome quadruble.*"

In cases with extremely hard stones, too hard for crushing with the common instruments, the bilateral section has been recommended, though others preferred the sectio alta, or even *recto-cystotomy*. Performing the bilateral operation the arteria transversalis is almost always, and the bulbo-urethralis occasionally, cut. I have seen the latter happen once, and a terrible bleeding to ensue, which the operating surgeon had the greatest difficulty in arresting. The rectum in the cavum ischio-rectali is also considered

liable of being injured, and a fistula remaining here gave formerly a great deal of trouble in its cure. With the increase in size of the calculus the danger of laceration of the bladder is said to increase, and mostly so shall such be the case in the presence of an alkaline urine, when phlebitis of the *plexus venosus prostaticus* might be followed by pyæmia. It is only by an early and thorough asepsis, or antisepsis, with a proper drainage of the bladder during three or four days, that a thus endangered life may be saved.

One of the main objections against the *lateral section* is based upon cutting the vas deferens and vesiculæ siminales of at least one if not both sides, followed by sterility. If injuring these parts should really take place, then the danger will certainly be increased by performing the bilateral cut. But the opinions upon that point are still divided, notwithstanding the many times that the operation has been performed.

Secondary hæmorrhage, if it should happen, will mainly present itself with the patient coming out of the narcosis, when the bandages are removed and free motion of the limbs reëstablished. A sudden cessation of the flow of urine ought to be taken as an indication of either a second calculus, or coagulated blood. Under all circumstances the surgeon must ascertain the real cause and convince himself that no other stone has been left behind.

Epicystotomy, Sectio Alta, Cystotomia Supra Pubica.—This method has also been highly recommended, especially in cases of children, on account of the peritoneum standing higher at that early age, than in later years; it being therefore out of the way to be wounded. The operation has also been favored in presence of extremely large stones. *Astley Cooper* and different other distinguished surgeons considered the operation associated with a great deal of danger, fearing not only of injuring the peritoneum but also of an infiltration of urine into the interstitial pelvic tissues, followed with suppuration, pyæmia, and frequently with death. They place this method on an equal footing with the median operation; and a dangerous infiltration which might happen, induces them to doubt its superiority over the lateral section. As if not under equal circumstances, as in presence of a decomposed, or, alkaline urine, the danger from infiltration would be as serious in the lateral section as in the sectio alta. Since the objections raised against this operation cannot well be maintained, on the contrary, the method under circumstances, as in the presence of extremely large stones, or, their enclosure in diverticles of the bladder, may be desirable, whilst the comparative freedom from danger, if properly executed, will moreover enhance its value, I intend to give here such directions for its practical execution, as will render the method perfectly safe.

I omit the initial steps of the operation, as cleaning of bowels, narcosis, position, etc., as sufficiently known. The metallic bougie used here ought to have an inner slide, and should not be introduced before a deep narcosis has been obtained, an advice which holds good also in other methods where a bougie or catheter is to enter the bladder. The best way to avoid injuring the peritoneum, consists in elevating the bladder by filling it to some extent with warm water, and likewise also the rectum after the narcosis has been effectuated. Under an elongation of the urethra the bladder and plica Douglasii are raised, and sufficient room gained between the peritoneum and symphysis ossium pubis for the performance of the operation. The plica peritonei hereby retires to the extent of three centimetres from the symphysis, so that a clean cut can be made into the bladder. These three centimetres of the bladder are only covered by the fascia transversalis, which is recognizable and characterized by the fat enclosing that membrane even in lean persons, whilst it forms a thick layer in fatty persons. Removing that portion of the fascia from the anterior wall of the bladder, instead of incising it, would be committing a great blunder. *Rosir* has very properly warned against it.

In performing, therefore, the operation of epicystotomy, and to secure success, the following steps have to be strictly and minutely executed: After the catheter has been introduced, the bladder is nearly filled with warm phenol water, of 3 per cent., to retain which the catheter ought to be provided with a faucet. The bladder in this manner being washed out three times with the phenol water before finally being filled, the catheter is removed and exchanged for the properly constructed bougie, with an inner slide. Should the liquid escape at the sides of the instrument, an assistant has to compress the urethra around it. The point of the bougie is pushed upwards right above the symphysis ossis pubis towards the anterior wall of the bladder. An incision is now made in the linea alba between the pyramidales, in a downward direction, through the abdominal coverings, till the bladder has been reached. When the point of the bougie is distinctly felt, two small hooks are inserted into the bladder on both sides of the point of the bougie and held there by assistants. The bladder, with its covering fascia, is incised from the point of the bougie in a downward direction till the left index finger of the operator can be introduced, which finger then takes the place of the bougie, which is now retracted, the finger instead hooking up the bladder. That finger is not to leave the bladder till the operation has been finished. In opening the bladder some of the phenol water will of course escape, but having the bladder still secured by the hooks, a blunt-pointed bistouri is introduced, under the direction of the finger, and the bladder further incised downwards.

The stone is now searched for, and when found, detached, and if not too large, it is easily grasped with the forceps, assisted by the finger, and extracted. Should the calculus be inclosed in a diverticle, it has to be detached carefully. In case of an extremely large calculus, the lithotriptor, or lithoclast of Luer, will be employed to crush the stone. In order to obtain a larger opening, it has been proposed to cut the bladder in a transverse direction above the symphysis. But herewith the inner pyramidales would be divided, causing a disposition to a subsequent abdominal hernia.

In this method we have, therefore, the means of removing the largest as well as the smallest calculi

with comparatively little difficulty. Nevertheless, in case of a catarrh, or of other complications of the bladder, as a cystitis or pyelonephritis, with a decomposed urine, and where drains have to be applied, the propriety of performing the operation of epicystotomy may well be questioned. The necessity of keeping the bladder empty would expose the patient for some time to the most uncomfortable abdominal position (though such has been recommended).

In the presence of a very extensive middle lobe of the prostate, when *posterior catheterism* should become necessary, or even external urethrotomy be required under such circumstances the method would certainly have no superiority over the pærenial median of Allarton, since the bladder has to be kept in a constant aseptic condition. In uncomplicated cases, with an acid urine, where no interstitial infiltration need be feared, the wound could be farther attended to.

Here it has been recommended to sew up first the bladder, and in applying the sutures very close to each other, to facilitate "a healing by first intention." In thus sewing up the wound, the sutura nodosa with well carbolized silk, ought to be applied, but taking care not to enclose the mucosa in the sutures. If by filling the bladder now with phenol water, it be found water tight, then the abdominal coverings, such as skin, muscles and fascia, may all at once be enclosed in that second row of sutures. But a successful healing of the wound of the bladder by first intention being so difficult to obtain, since it requires a very accurate application of the wound's edges, the latter is better left to heal by granulation, under an exact appliance of an aseptic occlusive dressing. In such a dressing the scrotum and the upper part of the thighs ought to be encompassed, leaving out only the catheter in the urethral canal for injecting the bladder three or four times daily with three per cent. phenol, or, thymol water.

In case of a calculus being enclosed in a diverticle, it is alone the Sauverdin method by which the exhæresis of the stone can be accomplished without any laceration of the bladder. The operation further leaves no incontinence of the urine, neither is a subsequent sterility to be feared. The bleeding is but a minimal one. A hypertrophy of the prostate is irrelevant and in strictures of the urethra, be they spasmodic, or impenetrable, there is certainly next to Allarton's method no better operation recommendable.

There is left for me one more operation belonging to this division, to say a few words about the

Procto-Cystotomy.—Cystotomia recto-vesicalis.—Of some eighty reported cases up to the year 1881, the mortality has been 1 in 5.3. If more were required to assign that method its proper place in surgical practice, it might be mentioned that the ever practical Malgaigne, in the latest edition of his "Operative Medicine," has totally relegated that operation from its contents. That ought to be sufficient for its condemnation; nevertheless, let us see if something could not be said in its favor, the more so since it was once held in especial appreciation by that distinguished Italian surgeon, Vacca Berlinghieri, who favored it on account of its easy execution and being an almost bloodless operation. My friend L. Bauer, of St. Louis, formerly of Brooklyn, N. Y., who performed that operation in 1859 successfully and for the first time, as he said, that the operation had been performed in this country, also claimed for it a superiority over all other methods. Bauer was no doubt led to that opinion like Berlinghieri, by its easy execution and successful issue; but such are not the only, nor the most important factors, in assigning a method its proper place in surgical practice.

If we distribute the rectum according to the anatomist Hyrtl, into three sections, then the second or middle section will extend backward alongside the os coccygis and sacrum, and in front from the prostate till to the plica peritonei, or, Douglasii. Its length is variable. With an empty bladder it measures from 5 to 7 ctm. (2″–2½″). If the bladder is extended by being filled with water, it takes hold of the plica Douglasii, or, that place where the peritoneum coming from over the fundus of the bladder is attached to the rectum. It will be moved somewhat higher up and correspond with the concavity of the sacrum. The total length of that section in its anterior portion is in contact with the fundus of the bladder, while at its sides the seminal vessels are met with. The easy injury of the latter by the recto-vesical cut and perhaps a deep position of the plica Douglasii, whereby the peritoneum may be injured, was the cause that one of the former defenders of that method, Sanson, again relinquished it. It is moreover an undeniable fact, that in elderly people the peritoneal duplicature of the rectum descends further down than commonly, and that in large stones the incision, if extended too far upwards, may come in contact with that membrane and even open the abdominal cavity, to say nothing of cutting the seminal vessels and causing sterility. Another salient point in its disfavor, consists in the inability of the surgeon to apply a proper aseptic dressing, nor is it always in his power to protect the wound against a sudden descent of fœcal matter. A remaining recto-vesical fistule, which has also been mentioned among the objections, has to-day lost its importance, since its cure, or failure of being cured, depends exclusively upon the skill and dexterity of the surgeon. Regarding the material used here for sutures, it does not matter whether silver, annealed iron wire, or carbolized silk is used. It was a mistake that I, with many others in this country once committed, to think that metal wire possessed a special virtue. It was an error, since properly prepared carbolized silk will serve the same purpose as wire; and "the new and lasting impulse in lithotomy" which Bauer ventured to predict upon the potency of silver wire sutures has not only not been verified, but they were not even able to save the operation in question from being thrown into the waste basket. Even in modifying the operation, by placing the highest point of the incision not further up in the rectum than about 2½–3 ctm., altering thereby the cystotomy into an urethrotomy, securing the peritoneum against any possible injuries, even then would the impossibility

of applying an aseptic dressing be sufficient to doom the method. In complicated cases in the presence of a cystitis, of an alkaline, or decomposed urine, when the wound must remain open and a healing of it per primam could not be thought of, the constant exposure of the wound to being contaminated with fœcal matter, not to speak, moreover, of the interstitial infiltration of the urine and its life-endangering consequences, would offer sufficient reasons to relinquish the operation, even in its modified form.

Calculus in the Female Bladder.—Calculi in the female bladder are also of renal as well as of vesical origin. The interior of some of these stones is either oxalates or urates, while its greater portion, the covering, consists of phosphates. It also happens that the nucleus may be a foreign body, such as a hairpin, a straw, etc., which in most instances found their way into the bladder through the urethra, as in children playing with such substances. In one case, that of a young lady, a hairpin which had been swallowed accidentally, nine years previously, and which finally became located in the bladder, and in part incrustated, was extracted with a dressing forceps.

The singular incident that calculi in females have been and are still occasionally overlooked, may find its explanation in a careless examination, but also by the fact that uterine disorders frequently accompany an excitability of the bladder, and it thus happens that such calculi are at first taken and treated for uterine disorders. In all such obscure cases a thorough examination of the bladder ought never to be overlooked, and this is best executed by the finger of one hand in the rectum, whilst the other hand carries the bougie, or presses deep into the abdominal coverings above the symphysis pubis.

The removal of a calculus from the female bladder, if not executed through the artificially, gradually dilated urethra, either entire or after having been crushed by lithotriptic instruments, is best accomplished in cutting through the anterior vesico-vaginal partition wall, a method formerly much feared on account of a remaining fistula, for a long time considered one of the approbria in surgery, and, singular to say, from the mere failure of a properly constructed speculum by which the parts could be made more accessible to the operator. And what is still more singular, when that speculum had been finally discovered, in Germany, and its description was accompanied by six successive cures of vesico-vaginal fistules by the German Metzler, it was overlooked till it came back from the new world to the land of its invention under another name, and encumbered with some paradoxical paraphernalia fully documental of its real origin. The silver-wire sutures, divested at a later date of their imaginary attributes, were replaced by carbolized silk, without altering the success of the operations. Vesico-vaginal fistulas are to-day no longer feared, and their cure may be predicted with an absolute certainty.

In order to examine the interior of the bladder with the finger, in case a caculus is enclosed in a diverticle of the bladder, in "*a vessie à colones*," the late Professor P. Simon, of Heidelberg, Germany, invented a set of specula made of hard rubber, and secured with a conical obturator or mandrin. These specula, of seven different sizes, from nine to nineteen mm. in diameter, he introduced one after the other, and became thus enabled to dilate the urethra to a lumen of two centimeters, after having incised previously the mucous membrane of the meatus urinarius by slight cuts of one-quarter to one-half centimeter deep, in four opposite directions, to avoid its laceration. Notwithstanding these forced dilatations at so short intervals, Simon never saw either a paralysis or an enuresis follow, the urine being always retained. In this manner a palpation, or illumination of the interior of the bladder could be executed, not merely in case of stone, but also in other diseases, like hypertrophy or tumors of the bladder. As a matter of course, these dilatations must only be made when the patient is in deep narcosis, to avoid the pain, as well as the spasmodic contractions of the sphincter vesicæ and the muscular fibres of the urethra.

I will here add a chapter of great importance to the surgeon, not merely on account of its intrinsic value, its relation to the parts which hereafter come into question, but also on account of the many existing errors committed by diverse authors. It is the *topographical anatomy af the pelvis minor.*

The existing errors and confusions in the description of these parts of the human body, comprising the pærineal region, and the interest every surgeon must feel for a correct information, induces me to write this chapter, and the more so since I have dissected these parts more than once, and in part as specimens for the anatomical museum of the University of Marburg. Seldom did I meet with a truer description than that given by the great anatomist, Hyrtl.

Well says the Nestor of anatomists: "The pærineum in the pen of authors has become a true Proteus, producing a mess which it is difficult to unravel." Considered externally, we find the pærineum limited by the root of the scrotum, the anus and the tubera ischèi, or those furrows caused by the thighs. The arcus ossium pubis is the skeleton of this region. The pærineum has three aponeurosès, or fasciæ pærinei. Its upper aponeurosis is situated underneath the subcutaneous cellular tissue, and ought to be named *fascia pærinei super-ficialis.* It is nowhere attached to the skeleton, though Quain, says Hyrtl, inserts it at the arcus ossium pubis. This aponeurosis comprises merely some indifferent blood-vessels and nerves, and is without any surgical importance.

The next or middle aponeurosis is the *fascia pærinei propria.* It is attached posteriorly to the levator-ani, os coccygis, and the tubera ischèi, anteriorly to the arcus ossium pubis. Being strongest in its fore-part, it is perforated by the pars membranacea urethræ of 20 to 25 mm. in length; behind it is perforated by the rectum.

The lower aponeurosis, which is commonly named the *fascia pelvis*, forms the basis of the pelvis minor, and is perforated by the organs passing through the pelvis. It is united with the levator ani. The anter-

ior portion surrounds the neck of the bladder and prostate, as the ligamentum pubo-vesicale and prostaticum, originating at the posterior part of the symphysis pubis. Another portion of it, attached to the tubera ischèi, forms the ligamentum ischio-prostaticum.

The last one, coming from the sides of the smaller pelvis, is the largest and most complicated one, and is known as *processus falciformis*. It has two layers, the inferior, which goes to the bladder and to the anterior portion of the rectum, and is called fascia recto-vesicalis. The strongest portion of this aponeurosis is adherent to the prostate and to the fundamentum vesicæ. The superior layer is covered by the peritoneum, which is connected with it by a loose cellular tissue. Above this aponeurosis the cystotome ought never to be entered. In cutting into the raphe of the pærineum, in order to open the membranous portion of the urethra, we will have to divide the anatomical layers in the following order: *cutis, subcutaneous cellular tissue; panniculus adiposus; fascia pærinei superficialis; plexus venosus* (inferior hemorrhoidal veins); *pars membranacea*, or, *isthmus urethræ, surrounded by Guthrie's and Wilson's muscles* (*musculi pubo-urethrales*).

So much for this concise, classical description of Hyrtl, of the correct anatomy of those parts of so important an interest to the practical surgeon.

The Urethra.—In the previous chapters it occasionally happened that further explanation of a subject was promised subsequently. If under this and the following headings, therefore, some repetitions should occur, I hope they will be excused as unavoidable.

A very important element in the nature of the urethra is its dilatation, its expansion. A healthy urethra, as stated by Dr. Otis, one of the distinguished American surgeons (and also present at the International Medical Congress) has stated the caliber of the urethra to be about 32 mm., whilst its meatus is given to be 24 mm. in circumference. Bigelow employed usually a catheter of 29 mm. (18 mm. engl.), which means the caliber of a well-extended urethra. Otis further stated that if the flaccid penis, *i. e.*, its pendulous portion measured 75 mm. (= 3″ engl.) in circumference, the urethra would have a capacity of 32 mm., and so on; so that 6¼ mm. (or ¼″) increase of circumference of the penis at or about the middle of the pendulous portion, would indicate an increase in size of the urethra of 2 mm. The range of the normal urethra, as demonstrated by measurements (done by Otis with his urethrometer), in several thousand cases has been given from 28 to 45 mm. in circumference, while the normal caliber of the urethra, as claimed by surgeons of all parts of the world, did not formerly exceed 21 mm. in circumference. In England and America, till Otis undertook these measurements with his urethrometer, the caliber of the urethra was accepted to be from 17 to 18 mm., and in France 21 mm. In one hundred carefully measured cases Otis received, in the average, 31.84 mm. when more or less strictures had been present, and 32.93 mm. in urethra supposed healthy. It will thus be seen that much larger instruments may be used in passing them through the urethra, as in lithotripsy, without fear of a rupture of its mucous membrane, than has been heretofore supposed.

Otis has given comparative normal measurements, but they, of course, do not indicate the maximum of dilatation which the urethra may undergo under certain circumstances, as for instance, under a gradual extending power and when a further portion of the urethra has been incised near the opening of that organ through which an entrance into the bladder is forced, or through which a stone is to be removed. That this is the main and most important question in performing the pærineal median operation of Allarton, or urethro-lithotripsy is self-evident. From my statements and measurements given in the case operated upon with Allarton's method of the stone, including the forceps, which both together had passed through that part of the urethra from the neck of the bladder to the beginning of the incision, without causing the least laceration, the circumference of the extended urethra measured 88 mm., instead of the circumference of a normal urethra after Otis from 28 to 45 mm., or of a hundred carefully measured cases in the average from 31.84 to 32.95 mm. in more or less healthy urethras. The extension of the urethra in my case, large as it appears to be, certainly does not prove the utmost dilatation of that organ, since not even a sign of blood became visible on the finger, introduced after the extraction of the calculus.

The above-mentioned measurements of Otis were given by him at the International Congress, and from what I have stated here, we can understand the success of Bigelow's method with his large lithotriptor and evacuating sound.

Previous to beginning the operation of external urethrotomy it is necessary to make a thorough examination of the urethra, as well as of the instruments. About 8–10½ ctm. or 3–4 inches from the meatus urinarius, what Otis calls the peno-scrotal angle (the beginning of the pars pendula penis), we meet frequently, after Otis, small bands. These bands, as well as all other localized constrictions of the urethra, from whatever cause, ought to be removed, either by a gradual dilatation, or internal cutting, so the instruments shall not be clogged, says Otis. The meatus, when much narrower than the balance of the urethra, has first to be widened by small cuts through the mucous membrane, as already mentioned. But all of these precautions and actions are of value only in a subsequent lithotriptic operation, whilst they are indifferent to either urethrotomy, or urethro-lithotripsy with an incision beyond those bands or localized constrictions.

Construction of the Catheter.—I consider it a fit place to call attention here to a new form of catheter which the late Prof. Hueter recommended as based upon the anatomical structure of the urethra.

Hueter, by whose recent and quite unexpected death surgery has suffered a heavy and irreparable loss at our time, called attention in his monumental work on surgery (one of the best if not *the* best work on surgery at present in existence, and with which he has

set himself a most brilliant and imperishable epitaph), I say Hueter called attention to the efficiency of Stearn's instrument for the dilatation of strictures of the urethra (Stearn's dilator is an American invention). The fact that he could pass that instrument, consisting, as known, of two movable flattened wires, through strictures through which he could not pass a round instrument of the form of our common metallic bougies or catheters, and of the same diameter, induced Hueter next to have given his catheters a similar elliptical form, and he had the satisfaction to experience the great benefit thus derived from that modification. But Hueter soon after also discovered the theoretical reason for this serviceable alteration. When the urethra of a dead subject is filled from out of the bladder with a paste of gypsum, and after the paste has hardened and the soft parts have decayed, a proper impression is obtained of the form of the urethral tract, the normal anatomical construction of the urethra, thus obtained, of the part corresponding to the glans penis presents a perpendicular cut, while the balance from the fossa navicularis backwards, including the pars cavernosa, membranacea and bulbosa, are transformed into a horizontal split, in which the upper urethral wall lies attached in close contact to the lower one. The middle part of the urethra, which is widest, will permit the passage of a common round bougie as well as of one flattened or oval, but towards the pars membranacea the channel obtains more the form of a reclining oval.

In the handbook of Billroth and Pitha (Bd. III, 2d div., 8 part, page 41) Socin has given the urethral split in the form of an inverted T (⊥). Hueter's oval male catheters have two eyes near the point, one at the convex, the other on the concave side of the curvature. The entrance to the channel of the catheter is the same as on the common round one. That the new form is easier of introduction and causes less irritation has been observed by all persons who have tried both forms upon themselves. The great facility and disencumbrance of introduction was surprising to all of the old habitués using the catheter. "But there is even a time for the specialist, when the urethræ of the patients have to pay for the apprenticeship," says Hueter. "The number of these sufferers would certainly be diminished with the use of the oval catheter." Even in the swelling of the prostate, which is the most fruitful cause for a punctio vesicæ in retention of urine, this form of the catheter might be of great service in reducing the mortality from that operation, which according to a careful collection has been given at 13 per cent. Hueter asserts that in such a swelling of the prostate, his oval catheter would enter easily, when the round one would not enter at all, displacing the middle lobe of the gland to make room for the instrument to enter the bladder, by adapting itself better to the small horizontal split, thereby preventing its point from entering the tissue of the gland and causing "false passages."

During 32 years of professional practice in this city, I have been obliged but twice to perform puncture of the bladder, among dozens of cases of retention of urine, from an extensive increase of the middle lobe of the prostate. Both cases recovered, though they happened in the beginning of my practice and when no aseptic treatment was thought of. I was also present when a surgeon, by pushing the trocar through the rectum, not only failed to enter the bladder, but, singular to say, postponed a second attempt for the next day (!), when he had the misfortune of meeting with a corpse. The man, colored, about 55 years old, died during the night of violent peritonitis. At a later period, after I had become acquainted with the wonderful efficiency of the galvanic current in the treatment of strictures and obstructions caused by an hypertrophied prostate, it never happened to me again to have recourse to the forcible and rude means of the use of the trocar in similar conditions, but always succeeded with patience and the use of the proper pole of the battery attached to the bougie or catheter, to enter the bladder.

[TO BE CONTINUED.]

DELIRIUM TREMENS.

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A Clinical Lecture Delivered at the Good Samaritan Hospital, Cincinnati, December 1, 1884.

[*Reported by James M. French, M. D., Assistant to the Chair.*]

GENTLEMEN—The resident physician, Dr. Carter, will read us the history of the case:

"*History*—Thomas D., æt. 48, a laborer, has been a hard drinker for twenty-seven years. For a period of five or six years just preceding the present year, he abstained from drink, but during the year just past he has indulged very freeely. He has had two attacks of delirium tremens—the first in 1861, the second in 1865. During the first attack he says that he imagined he was lying in a bed of serpents, and that the room seemed to be filled with demons of the most fantastic shapes. At an unguarded moment he ran from the house, pursued, as he imagined, by a huge snake that seemed to crawl through his clothing, and was endowed with articulate speech. The patient was admitted two days ago, and stated that he had had an attack the previous night. In the attack his room seemed to be filled with various animals, as dogs, cats, rats, birds and insects, which sported about and seemed to get into his bed, and crawled about his person. Yet he was able to cause them to disappear by kicking them off. He also saw figures of gigantic men, who appeared about to fall upon him. He heard, too, numerous voices reproaching him, so that he was unable to sleep. His mouth, he thought, was filled with grass, which he could not get out, and with the grass he thought he was trying to spit out devils. During the day of his admission he was pretty quiet, and he rested pretty well the following night. Yesterday, however, he could not be restrained, but wandered about the ward in an indifferent, abstracted mood, appearing unconscious of his movements. So marked was this that while he

was standing before the fire the sheet in which he was wrapped became ignited, and was torn from him without arousing his attention. After this accident he was secured in bed. Toward evening he fell asleep, and rested quietly all night and the greater part of this morning. His appetite has been ravenous, almost insatiable during the course of his illness. At present he is fully conscious of all his actions and surroundings."

I asked the resident physician in preparing the history that he has just read to record just what the patient said, and he has given us a very succinct account of the delusions which are common in these cases. I wish you could have seen the patient in the height of the attack. for you would then have seen a picture that you would never have forgotten. Now he lies quiescent, and is not affected even with tremor; there is no tremor of the hand or of the tongue. Nevertheless, I think it is perfectly plain to you that we have had to deal with a case of delirium tremens. It is generally pretty easy to recognize one of these cases, but every now and then a grievous mistake is made. Not infrequently an individual is picked up on the street, and perhaps carried to the station-house, thought to be suffering from delirium tremens, and it turns out afterwards that he has had a sunstroke, is apoplectic or uræmic, or has received a blow upon the head. Only a few years ago a man was found propped up in a door-way in this city, like the hunchback in the story. He was carried to the station, and died in a short time. When the body was examined the next morning, it was found that the individual had been shot through the brain.

Then a man may have delirium tremens that is not due to the excessive use of alcohol. A man may show delirium and a tremulousness that is characteristic of the disease before us, in any acute infection, especially in typhoid fever and in septicæmia. Neither does alcohol give every man delirium tremens, or the affection would be a great deal more common than it is. It comes on, as a rule, after or in the height of a debauch. This man has been a hard drinker for a great many years, but this is only the third attack he has had. I have no doubt that he has been found in a condition of acute alcoholic prostration a number of times, but that is a quite different condition from delirium tremens. The effect which we here observe is produced by the direct action of the alcohol on the brain. In all cases, in fact, alcohol expends its force upon the brain; first upon the great brain, then upon the centres of motion at the base, then upon the cerebellum. The centres of respiration are last but most profoundly affected, and when a man dies from alcoholic poisoning, he dies of apnœa. He literally chokes to death. We see this from the cyanosis, from the suffusion of the face and surface. Often we are called upon to decide whether a man has acute alcoholic coma, uræmic or apoplectic coma, or is suffering from an injury to the brain. So it behooves you well to know the signs of delirium tremens.

Perhaps you know that it is a disease that is not so common in some countries as in others. It is most common in cold climates, as in Norway, Sweden and Denmark, because in these northern countries there is a constant craving for stimulating drink, not felt so much in warmer climates. In cold climates the tissues are consumed more quickly, and this accounts for the constant craving for alcohol. In warm climates, where wine and beer flow like milk and honey in the Promised Land, you will scarcely meet a case of delirium tremens. In these drinks the alcohol is in much less amount. Brandy has 60 and wine and beer but 2 to 6 per cent. of alcohol.

Thomas Sutton, an English physician, was the first to give a distinct description of this disease. He taught us that there are three characteristic symptoms to be looked for, namely: delirium, tremor and insomnia. So it is sometimes said that delirium tremens is a disease which stands upon a tripod. There should be added a fourth symptom, a symptom which, however, is rather a negative than a positive sign. Delirium tremens is not accompanied by fever. When we find all these four symptoms present, delirium, tremor, insomnia and apyrexia we can have no doubt of the character of the case.

For the most part an individual who is attacked with delirium tremens has it come upon him suddenly. Not infrequently it is prefaced by or associated with nausea, vomiting, anorexia, etc., then the individual becomes delirious. You heard all the phenomena of the delirium in the history read by the resident physician. The individual sees curious objects, and all kinds of animals, strange to zoology and comparative anatomy, serpents and dragons, such as might be read of in the "Arabian Nights" or fairy tales. He tells us now that he was in his earlier attacks, pursued by snakes. I doubt very much whether he really saw snakes at all when he was in the delirium. It is more usual for these individuals to see insects, small animals as mice, rats, beetles and bugs, that crawl all about them, fill the room and for the matter of that all inter-planetary space. Or they may see objects about them transformed into objects of horror. The bed-posts become transformed into huge dragons, and a chair is changed into a demon. More especially the patient fears his fellows. Assassins, he imagines, are standing at his door or crouching under his bed waiting to kill him. He jumps out of bed and in his alarm, perhaps, escapes from the house. I have known a physician going through a ward with a stethoscope in his hand, beaming with benignity, to be mistaken by one of these patients, for a robber or a murderer, the stethoscope mistaken for a weapon, and the patient jump from the bed and run for his life. These individuals are not permitted to rest by reason of their perverted fancies. They are just about to fall into a sleep when they are awakened suddenly with a start, a shudder, or a shriek at the demons which have been meanwhile conjured up. It is not that the patient wants to injure any one, it is fear that characterizes his attack. The physician and attendants are mistaken by him for assassins, and from these he desires to escape. It is not, as a rule, I say, a maniacal onslaught that they make, but everything with them is the attempt to escape from danger. This is the characteristic of delirium tremens, horror and fright and fear. You

see the expression of terror in the dilated pupil, in the general physiognomy that is unmistakable.

After or with the stage of delirium, comes the tremor; tremor of the hands, of the tongue, of the jaws, and agitation of the extremities. The tremor of the hands is more marked than in paralysis agitans. It is a tremor, too, that is manifested when the individual is quiescent. This man can now hold his extended hand quiet as you see, but even for a long time after the individual has recovered, volitional movements are marked by feebleness and tremor. The tremor also affects the tongue, so that the individual has difficulty in speaking; he disarticulates his words. You ask him to put out his tongue, and it is with difficulty that he obeys you. Tremor is, in in fact, a symptom so prominent and so persistent that it has baptized the disease with its name.

Another of the more important symptoms is sleeplessness. The individual would fall into sleep from exhaustion, were it not for the fact that he is constantly aroused by his fears. If he falls asleep, he awakes, as I have said, with a start or a shudder. Again he falls asleep, but only to be again awakened with a start, and to see the room filled with objects of terror. To what extent this insomnia destroys the rest of the patient, you can learn from the enormous doses of hypnotics we have to administer in order to give him rest. We often have to give doses that compel us to stand by the bed-side and watch the patient until the action of the remedy has to some extent worn away. Thus we sometimes incur the superadded danger of narcosis in our attempt to give the patient sleep.

You take the temperature in a doubtful case and you find that it is normal, or if you take it as the full action of the alcohol has expended itself, you may find it slightly below the normal. You know that alcohol reduces temperature. We give it now in preference to quinine in septic diseases, because it reduces the temperature absolutely. Typhoid fever shows us a fever during the stage of delirium, and this you would also find in the history of septicæmia. There might be times in the course of a septicæmia when the temperature would be normal or subnormal, but as a rule you would find it elevated. You would also have in the history, the statement that the patient had had a chill or a succession of chills, so that you would have no difficulty in the diagnosis. You can not produce alcoholic intoxication in a case of sepsis. When alcoholism supervenes the disease, as a rule, is done. There are other points of great value in the diagnosis. In the first place the gastric distress sometimes enables you to make a diagnosis at once. You find that the individual's clothes are soiled, or the ground near where he lies is covered with vomited matter. You investigate the vomited matter; don't be afraid to put your nose down into the vicinity of it, and you detect the odor of alcohol, or rather of the etherial oils that accompany the alcohol. You put your head down close to the individual and you discover the odor of alcohol in his breath.

Another thing: These patients are brought by alcohol into a state of anæsthesia. Alcohol produces anæsthesia. In the days before the use of chloroform and ether, the surgeons, when about to operate, gave the patient a drink of whisky, and even now, in trivial operations, where it is not considered necessary to administer an anæsthetic, we give the patient a glass of good wine or whisky, and a kind of anæsthesia is produced. In delirium tremens this anæsthesia is plain enough. The patient who has jumped from a window to the ground gets up and tries to walk away with a cracked pelvis or a broken limb, unconscious of any injury.

We have now had signs sufficient in a diagnostic way to distinguish this disease from others that may simulate it. The delirium, the tremor, the gastric distress, the insomnia, the absence of fever and the anæsthesia distinguish it enough. Generally, however, the individual comes to you with the history of delirium tremens. He has, as a rule, been a continual drinker. The attack you see is not the first.

It is only a certain percentage of persons, as I have said, that have delirium tremens; and these persons are often victims of other neuroses. It is surprising how often epilepsy is associated with this disease. Westphal has shown us that 30 per cent. of these cases are epileptic before the disease comes on, and 30 per cent. are seized with epilepsy after it.

But there is no rule without exceptions. I have intimated to you that delirium tremens is a disease without fever. Now, fever is sometimes associated with it; but when this occurs the elevation of temperature does not belong to the delirium tremens proper; the fever is due to complications that are accompanied with fever. Perhaps the most common complication of delirinm tremens is pneumonia. This is due, to a great extent, to the exposure to which drunkards are subjected. The individual, in many cases, has fallen into a gutter, and has got wet, and a pneumonia supervenes. The pneumonia of drunkards is frequently a double pneumonia, and there is no pneumonia that is so fatal as a pneumonia during alcoholism—not only because it is so often double, but because the man's bad habits have broken him down, and destroyed his powers of resistance to disease. Next in frequency to a pneumonia is a pleurisy; but a pleurisy runs, as a rule, a much more favorable course. What you will learn to watch for with much greater interest is a meningitis. This is the complication that makes the individual the victim of maniacal delirium. You would observe, in a case of this kind, fever, and a contracted condition of the pupils, instead of the dilatation that is usual in these cases. Above all things you would lay stress upon the fever Now, because a man has a maniacal delirium, it does not imply that he has a meningitis, for an individual generally has maniacal disturbances in the excitement of his fear some time in the course of the disease. It is occasionally very difficult to restrain these cases of delirium. In the hospitals where they treat a great many such cases they have padded rooms, where the individual can pitch around as much as he likes without injuring himself; but here we have to use restraint, and sometimes it is necessary to put on the straight-jacket, on account of extreme violence.

Westphal has shown that six per cent. of these cases attempt suicide, but homicide is almost unknown. When it does occur, it is accidental, in the effort to escape. This man, we are told, wandered around the ward until he set himself on fire at the grate, and so profound was the anæsthesia that he paid no attention to the accident at all.

Suppose, now, that you should see an individual lying in the street in an acute state of intoxication, or coma, and it should become a question to what the coma was due, how would you decide? Well, in an attack of opium poisoning, you would notice the pupils contracted down sometimes to a pin-point, but you must remember that when the stage of asphyxia comes on in opium, they sometimes dilate. Next you would have the respiration reduced probably to only four or six per minute. This slow respiration is characteristic of both affections, because both poisons affect the medulla, but it is more marked when due to opium. Thus, with the pulse, pupils and respiration, you would draw a line between alcohol and opium, especially if you could get anything of the history of the case, or if you could detect an odor of either drug upon the individual. If you suspected uremia, you would at once draw some urine from the bladder and examine it for albumen. But many drunkards have albuminuria, and in such a case you could not distinguish the affection. As a rule, however, this complication does not cause any difficulty, and you could eliminate one or the other. Look for the dropsy of Bright's disease, especially under the eyes. As for injuries about the head, you would have to examine the head, of course. Most mistakes are made where the physician has taken it for granted that the case is one of alcoholic intoxication without examining properly. I know of nothing more humiliating to the physician than to make a mistake like this.

What will you do for a case of delirium tremens? I have already intimated to you that it is not, as a rule, a dangerous disease. Most cases recover. When they die, they die of complications, or those slow changes that are brought about by the continued ingestion of alcohol; namely, the destructive inflammation of the connective tissue of the brain, giving rise to progressive paralysis of the insane; or a chronic inflammation of the connective tissue of the liver, giving rise to cirrhosis. I have told you that it is necessary to use some restraint in these cases. Now you should use just enough force to keep the patient quiet, and not any more. If you have attendants enough, it is only necessary for them to watch beside the bed and to keep him from leaving it. It is more a matter of vigilance than of force. But that cannot be done, as a rule, because these are generally men who have run through with their fortunes and cannot hire attendants enough.

We have next to discuss remedial agents. The best remedy is chloral, but to have success, we must give it in large doses. It is better to give it in one large dose, as forty grains at a time, than in repeated small doses. Chloral is preferred to other remedies because it does not interfere with digestion and does not leave any traces behind. But then there are certain contra-indications to its use. You could not give an old drinker chloral; the alcohol has already produced changes in the heart that you must watch. The heart has become fatty, and the aorta atheromatous. You detect these changes in the pulse or by putting the ear down over the heart. Many and many a drinker has been pushed off with a dose of chloral. What then, in a case in which you could not give chloral, would be the substitutes? The best substitute is opium. Opium was looked upon as the sheet-anchor in these cases until the discovery of chloral. You shall give it in the form of morphia in the dose of quarter or half a grain under the skin that it be not rejected, and watch the effects. The individual as a rule falls of into a sound sleep, but you would hesitate to give the remedy too frequently. You may have to repeat the dose once or twice until the patient gets a grain or more, but you would watch the effect closely; you would watch to see if the pupils were contracted. If dangerous symptoms supervened, you would have to sit down beside the bed and keep the individual awake. You do not have to make him walk about to do that, but simply sit beside him and occasionally pinch his ear or slap his face or especially call him by name; usually this is enough. While you watch the patient count his respirations. As a rule you would not venture to leave him until his respirations were as frequent at least as ten per minute. Do not forget how boldly you may use atropia, in the dose of half a grain hypodermically, in a desperate case. Now, in a case where there was some sign of fatty degeneration of the heart, as there is in the case before us, you would certainly put the patient under digitalis. Grade your dose according to the condition of the patient; sometimes you will have to give as high as a tablespoonful of the infusion every two hours. Then there is a remedy which you would have the patient use as a drink, and it is in a mild case almost a specific, namely, coffee; you would have the individual drink strong coffee. Let him drink it without any milk, because the tannic acid of the coffee coagulates the albumen of the milk, and renders it less easily absorbed; or you might give the active principle of coffee, caffeine.

Binz found that coffee was an absolute antidote to alcohol, and that dogs saturated with caffeine could hardly be intoxicated with alcohol. So you would use coffee or caffeine, and you would recover your patient, as we have done this man within the short space of twenty-four hours. Lastly, if you choose, when you are all through you may lay aside the role of physician and assume that of the preacher. You can generally do more good than the temperance lecturers. You can call attention to the harm that alcohol does to his brain and his liver. These are the chief organs to suffer; but you are not hence to infer the alcohol is carried irregularly over the body; it is carried uniformly everywhere—absorbed always by the veins—never by the lymph vessels. Tell your patient that alcohol is one of those poisons that leave a permanent effect. Opium does not, and nicotine does not. A man may suffer the profoundest poisoning with opium and nicotine, and

yet recover entirely from it. A man may take either for years, but the moment he stops the poison is eliminated. But it is not so with alcohol. Alcohol produces the profoundest lesions in the brain and other organs, diseases that are called progressive. These things, I say, you can depict to the patient, and sometimes you can rescue him from the habit, but not as a rule, for he has become addicted to it for life, and is not capable of breaking it off, because the force of his will is broken, too. Most of the inebriate asylums of the country are failures. The individual can be sent to an asylum, and so long as there, kept under observation, he will do very well; but the moment he is out from observation, he relapses, because, as I have said, the regular drinker has lost his will. We do not expect to accomplish much by that method, or by our or other sermons; but we expect to accomplish more by good laws—by license laws, perhaps—by some means that will put the abundance of liquor beyond the reach of the majority of men. It was found in Russia that the amount of crime was in exact ratio to the tax that was put upon liquor. In the countries where people drink the milder wines, we do not find any of these diseases. So, if we can do no more, we may impress upon the patient the wisdom of taking only the lighter drinks.

PREMATURE DETACHMENT OF THE PLACENTA DURING LABOR AS A PROBABLE CAUSE OF STILL BIRTH WHEN NO OTHER CAUSES CAN BE ASSIGNED.

BY H. LANDIS GETZ, M. D. MARSHALTOWN, IA.

For some years past we have entertained the view that the premature detachment of the placenta in otherwise normal labors, was occasionally the cause direct of "still birth," or the cause of the enfeebled condition of the child at birth, where only the most prompt and efficient measures which can be brought into practice by the accoucheur, are the means of establishing respiration, a perfect circulation, and consequently the life of the child, which but for prompt measures must otherwise have perished. We believe that a child born where the placenta remains attached to the womb until the child is expelled from the mother never requires any assistance to promote or establish respiration, and that where such assistance is necessary there has been either a partial or complete *premature detachment* of the placenta, for a shorter or longer time prior to the expulsion of the child from the mother, and the circulation in the cord will be enfeebled or have entirely ceased in proportion to the length of time the partial or complete placenta detachment may have existed. Our attention has been recently called to this subject in quite a decided manner, through the occurrence of two cases, which were of a very striking nature.

CASE 1. A primipara, second stage of labor, lasted about two hours; labor normal in every particular; child well developed; heart action very feeble; no pulsation in cord perceptible; no effort at respiration; had hand upon abdomen over womb when child was expelled from mother; held hand in same position until removed to cut the cord, which was done within two minutes after the delivery of the child, and there was not the slightest contraction of the womb from the time of the pain which expelled the child to the time of cutting the cord, and when the cord was cut there seemed to be no necessity for a ligature, inasmuch as there was not the slightest hæmorrhage; prompt measures restored the child, which has since done well, and is now several weeks old and has not shown an unfavorable symptom. The placenta was expelled by the first contraction of the womb after the expulsion of the child, about five minutes elapsing between the expulsion of child and placenta.

CASE 2. A multipara, eighth pregnancy; labors previous all normal; present labor normal, except that the vertex caught upon the symphysis in such a manner as required the application of instruments to dislodge it, which was easily accomplished; conditions of child, etc., almost identical with case 1, above described, except that at least fifteen minutes elapsed before there was any voluntary effort at respiration, and at least half an hour before the respiration assumed any degree of certainty or regularity. This child also was well developed, and when last seen was to all appearances well in every particular. These cases to us, together with former observations of a similar nature, would, it seems to us, go far toward the establishment of the belief and fact of the possibility of the occurrence of the accident, and its effects as herein suggested. If our theory is correct, then arises the question: What are the causes of this premature placental detachment, or how can it happen? Several ideas suggest themselves to us.

1. Where there is very large quantity of liquor amnii, and where the head for a time completely obliterates the outlet, where the space between the child and the wall of the womb is greater than the thickness of the placenta, this space filled with liquor amnii, and now as the forcible contractions of the womb come on the placenta having the liquor amnii as a bearing instead of the firm body of the child, is driven into the fluid, and torn from its attachments.

2. Where the head is locked in the pelvis, and does not recede as each uterine contraction ceases, the relaxation of the uterine tissues may be so great as to leave a material space between the walls of the womb and the body of the child, so that when the womb again forcibly and suddenly contracts, and the bearing for the placenta is not immediately reached, the latter is in the meantime partially or completely severed from its attachments.

3. Where a condition (owing to certain position or relation of body of child to placenta) exists; for instance, if the lower extremities doubled upon the body of the child should happen to be a few inches apart, the placenta happens to be attached immediately opposite this gap, the womb contracts, the bearing to the placenta is not exactly equable, and, as a consequence, there is a partial detachment of placenta

at the point where there is practically no resistance or bearing.

What can be done to avoid these accidents, if it is possible for them to occur—and our theories in the main are correct—we confess we have little to suggest, the principal remedy perhaps being in prompt delivery in all cases where the womb is fully dilated and there is nothing to contraindicate it by the pelvic and external soft parts. When instruments are used, the pærineum should always be given ample time for yielding, so that it may not be unnecessarily lacerated.

Nov. 24, 1884.

MEDICAL PROGRESS.

OBSTETRICS AND GYNÆCOLOGY.

THE USE OF COLD IN LABOR.—Dr. Grognot (*Bull. Gen. de Therap.*) recommends the use of cold to produce and increase labor pains. It is well known that to palpate the abdomen in a pregnant woman, the hands must be of approximately the same temperature with that of the skin of the patient, otherwise if too cold the disagreeable sensation will produce immediate contraction of the muscular walls of the abdomen and prevent a proper examination. But, if persisted in, this may extend to the uterus itself, and give the sensation of the presence of a regular globular tumor, contracting, not from the pressure, but from the effects of the cold. There are uteri which contract upon the slightest touch, but they are rare, and pressure generally produces no such effect. In consequence of these observations, Dr. Grognot uses a wash basin or bucket of fresh water, into which, when it is deemed desirable to provoke pains or to accelerate labor, he places his hands for one or two minutes, and then applies them over the abdomen, and as fully as possible over the region which covers the antero-superior surface of the uterus. In a few seconds the patient complains of a pain and the hands are removed. It is sufficient to use fresh water, as a greater degree of cold would not hasten contractions to any greater extent. To this may be added manual pressure and manipulation. It is generally not necessary to use this method more than ten or fifteen times. The following are his conclusions:

1. The application of cold provokes marked contractions during labor.
2. They are normal in their effects.
3. This method is without danger to the mother or child.
4. It can be used independently and without instruments.
5. It can be employed during all the stages of labor, for the expulsion of the foetus and also for the removal of the placenta.

CHRONIC DYSENTERY TREATED BY VOLUMINOUS ENEMATA OF NITRATE OF SILVER.—Dr. Stephen Mackenzie has read a paper on this subject before the Clinical Society of London (*Med. Times*). The mode of procedure he adopted was as follows: The quantity of nitrate of silver to be used was dissolved in three pints of tepid water in a Leiter's irrigating funnel, which was connected by India-rubber tubing with an œsophageal tube with lateral openings. The patient was brought to the edge of the bed and made to lie on his left side, with his hips well raised by a hard pillow. The terminal tube, well oiled, was passed about eight or ten inches into the rectum, and the fluid allowed to force its way into the bowel by gravitation. The injection rarely caused much pain, and often none. It usually promptly returned, but when long retained it was advisable to inject chloride of sodium, to prevent absorption of the silver salt. Various strengths had been used, from thirty to ninety grains to three pints of water, but usually one drachm of nitrate of silver was employed. The treatment was based on the view that, whatever the nature of dysentery, whether constitutional or local, in the first instance, the later effects were due to inflammation or ulceration of the colon, which was most effectually treated, as similar conditions elsewhere, by topical measures. Sometimes one, sometimes two injections were required, and in some cases numerous injections were necessary; but in all the cases thus treated, many of which had been unsuccessfully treated in other ways previously, the disease had been cured. The cases narrated were: 1. In which the disease had lasted several years on and off; two injections were used and the case was cured in six weeks. 2. Second attack, duration uncertain; four injections used; cured in five weeks. 3. Duration two months; two injections used; cured in three and a half weeks. 4. Duration five years; one injection used; cured in three weeks. 5. Duration eighteen months; two injections used; cured of dysenteric symptoms, but remaining under treatment for diabetes. 6. Duration fourteen months; one injection used; cured in seven weeks.

Dr. Carrington said that this treatment had been tried in the hospital at Greenwich without any remarkable effect, but the injections had not been so voluminous as those used by Dr. Mackenzie, which might, perhaps, explain the fact. The colon was usually capable of holding six pints of fluid, and the three pints used in some of the cases might possibly have failed to reach the affected parts.

CHRYSOPHANIC ACID IN CUTANEOUS AFFECTIONS OF THE EAR.—Dr. Strequart (*Jour. de Med.*) reports five cases benefited by this treatment. The first case was in a patient 31 years of age affected for six months with an eczema of the scalp, and for fifteen days with acute eczema of the right ear—thickening of the skin, liquid exudation, purulent and sanguinolent otorrhœa. Ordered chrysophanic acid internally, in the dose of 2 centigrammes per day, and injections of a warm solution of boracic acid. In two days the discharge diminished, the eruption became paler and the skin thinner. Two days later there was desiccation and desquamation of the skin. The dose of the drug was increased progressively to 5 centigrammes a day, when the stomach no longer tolerated it. After eight days of treatment the

affection had almost entirely disappeared. Six months later there had been no recurrence. The second case was in a patient 15 months of age, suffering from impetiginous eczema of the scalp of six weeks standing, which for fifteen days had involved the ears. Two centigrammes a day were given, and in seven days the cure was complete. The third case was three years of age; affected for three weeks with impetiginous eczema of the chin and right ear; same dose; on the fourth day desiccation and peeling of the skin; on the seventh day, cured. Fourth case, a patient of four years, acute eczema of meatus and auricle of right ear; same treatment; cure on the seventh day. Fifth case, patient 35 years of age, suffering for four months from a chronic eczema of the auricles of each ear, following double purulent otitis. Was given 3 centigrammes of chrysophanic acid a day. Less redness about the mastoid and parotid region, and diminution of sense of burning of auricles and meati in two days. Notable amelioration in five days. Complete cure in twelve days.

Treatment of Amenorrhœa by Permanganate of Potash.—Drs. Ringer and Munell have used the permanganate of potash in 69 cases of amenorrhœa in solution or in pills, beginning with a dose of 6 centigrammes three times a day and increasing it progressively to 12 centigrammes. This treatment has given excellent results in young girls, 18 to 25, who were affected with amenorrhœa from taking cold. The treatment should be commenced several days before the time for the appearance of the menses. The results of this treatment have been no less satisfactory in women from 35 to 40 years, in whom the amenorrhœa was symptomatic of repeated confinements and prolonged lactation. On the other hand, if the amenorrhœa is in consequence of normal pregnancy, the treatment is perfectly inoffensive. It remains inert if the amenorrhœa occurs in the last stages of phthisis. In the first stages of phthisis, the permanganate may cause the menses to reappear, but it has no influence on the progress of the disease. Finally, in young girls where the establishment of the menstrual period is retarded, it has but little effect.

The use of the permanganate in solution is sometimes followed by nausea, vomiting and acute pain under the sternum. This drug does not seem to act by modifying the composition of the blood, for it acts equally well with the plethoric and the chloro-anæmic.

Ipecac in Minute Doses for Metrorrhagia.—Dr. Jules Cheron (*Rev. Méd. Chis. des Maladies des Femmes*) has used this drug in a large number of cases of metrorrhagia, without regard to the causes as to whether they were functional or symptomatic, and with marked success. Its use for this purpose as an emetic is nothing new—as producing a slowing of the circulation and cardiac pulsations; but in minute doses, where it does not produce vomiting, or even nausea, it modifies, according to Dr. Cheron, the excito-motor force of the spinal cord as with, but better than, bromide of potassium. He uses of powder ipecac, freshly prepared, 0.20 centigrammes, divided into twenty papers, and gives one every hour, except the hour which follows a meal. After having used the whole of the 0.20 centigrammes, the medicine is suspended for twelve hours, to be recommenced if the metrorrhagia has not been completely arrested.

Vesical Injections of Nitrite of Amyle.—Dr. Dittel, of Vienna, recommends (*Sym. Med. Annales Maladies des Organes Genito-Urinaires*) the use of injections of nitrite of amyle into the bladder when the urine is ammoniacal. He prepares three drops of nitrite of amyle in solution in 150 grammes of water, and for each injection he uses a spoonful of the solution in a litre of water, with which he washes out the bladder by the use of the double sound. The influence on the urine is very rapid, and after the first washing out the ammoniacal and insupportable odor is replaced by a very agreeable odor of ether.

Subcutaneous Injections of Caffeine for Adynamia.—M. Huchard uses, at the Hospital Bichat (*Jour. de Med.*), subcutaneous injections of caffeine as an agreeable and satisfactory substitute for subcutaneous injections of ether, in adynamic affections that succeed hæmorrhages. He considers the drug as preferable to ether from being less painful, and from having a favorable action upon the urinary secretion, and particularly upon cardiac contractility. This makes the injections indicated in all typhoid and adynamic conditions (pneumonia, dothinenteritis, post-hæmorrhagic anæmia, etc.). He uses the following formula:

Sodium salicylate, 3 grammes 10 centigrammes.
Caffeine, 4 grammes.
Distilled water, 6 grammes, or q. s. for 10 cubic-centimeters. Dissolve in a water-bath. Each cubic-centimeter contains 0.40 centigrammes of caffeine.

Sodium benzoas, 2 grammes, 95 centigrammes.
Caffeine, 2 grammes, 50 centigrammes.
Distilled water, 6 grammes, or q. s. for 10 cubic centimeters.

Each cubic centimeter contains 0.25 centigrammes of caffeine.

The tonic and stimulant action of caffeine is also utilized in the form of wine caffeine containing 0.10 centigrammes of the active substance to the spoonful, and preparations are also made from a West African drug called *Kola*, which contains more caffeine than does coffee.

But caffeine is not only a tonic and stimulant, it is also a cardiac stimulant which sometimes gives admirable results in the last periods of asystoly, when the heart no longer responds or responds but imperfectly, to digitalis. The drug will not act in affections of the heart and kidneys unless it is given in the large doses of 0.50 centigrammes to 2 grammes a day. To assure its solubility it is necessary to mix it with salicylate of sodium, or better, with benzoate of sodium, as in the following formula:

Distilled water, 300 grammes.
Benzoate of sodium, } each 5 grammes.
Caffeine, }
Two to six soup-spoonfuls daily.

The Action of Salicylate of Soda upon the Uterus.—M. Balette has shown (*Jour. de Médecine*)

the good effects which follow the use of this drug in dysmenorrhœa, where the salicylate of soda quiets the pains and facilitates the discharge of the menstrual fluid. A dose of from four to six grammes, taken in three divisions, gives excellent results, and often quiets the pains at the end of fifteen minutes or a half an hour. But the drug has also another property which is less known, a menorrhagic action. M. Bucquoy has observed this influence, and M. Balette cites several observations which show that the salicylate may provoke metrorrhagia in certain cases, or rather prolong the menses and increase the quantity of the blood-flow.

This action may be explained as in the cases where the salicylates readily produce other hæmorrhages, as hæmaturia, and particularly epistaxis; as also vesical, pharyngeal, gastric and intestinal hæmorrhage.

The production of abortion has also been attributed to salicylate of sodium, but the evidences are contradictory. With several patients it would seem as if the abortive action was well manifested; with others, on the contrary, it had no effect. In M. Balette's experiments upon animals, the drug had no oxytocic action, and it would seem not to be abortive when given in moderate therapeutic doses. But as there are women who are predisposed to abortion, it is advisable, during gestation not to give the salicylate of sodium without proper cause, and to watch carefully the degree of toleration in the individual to the drug.

MEDICINE.

ON AGRAPHIA.—Dr. A. Pitres (*Revue de Médicine*) considers this subject at some length. He gives Charcot's definition, that agraphia is aphasia of the hand, as the most complete and concise that it is possible to apply to it. The word agraphia was first introduced to science by M. W. Ogle in 1867, but Marcé in 1856, was the discoverer of agraphia. The trouble with his work is that it is not founded on the study of simple and precise clinical observations. At present we recognize for the purpose of writing the combined use of several factors, viz.:

1. Visual memory, which gives us the remembrance of the form of letters, and their relative value in their innumerable associations in syllables and in words.

2. Auditory memory, which gives us the remembrance of sounds and their value in phonetic language.

3. Motor memory, which gives us the remembrance of efforts and muscular coördinations necessary to trace written characters correctly.

Dr. Pitres illustrates these assertions by detailing some well-defined cases, and arrives at the following conclusions:

1. There exist close analogies between the physiological mechanisms which preside over the formation of speech and of written language. There exist equally marked resemblances between the pathological disturbances which alter their functions.

2. It is proper, then, to distinguish three forms of agraphia, corresponding to the three classic forms of aphasia as known at present. These are:

A. *Agraphia by verbal cecity*, in which the patient cannot copy after an example, while he can still write his own thoughts, or from dictation.

B. *Agraphia by verbal surdity*, in which the patient cannot write from dictation, while he can still copy from an example or write out his own thoughts.

C. *Motor agraphia or graphoplegia*, in which the patient cannot write at all.

3. Each of these forms of agraphia has been observed as isolated and distinct in patients whose intelligence was normal and whose motility was intact. Several very precise observations of verbal cecity and of verbal surdity have been put on record. Observations of motor agraphia are rarer, but two at least of such cases are recorded at present as demonstrating this condition satisfactorily.

4. In the most of these pathological cases agraphia is associated with hemiplegia or with aphasia. In such complex conditions, the diverse symptoms to be found in the same patient, must be considered as coexistent and not subordinated one to the other. The complexity of the semiology in each particular case depends solely upon the variable topography and the greater or less extension of the cerebral lesions.

UREA AS A POISON.—MM. N. Grehant and Ch. E. Quinquand (*Jour. de l'Anat. et de la Physiol.*) have published the results of a large number of experiments upon various animals by which they reach these conclusions:

1. Pure urea is a poison which, in sufficient doses, produces convulsions and death, supervening after a variable length of time, dependent upon the dose employed.

2. The mortal toxic dose in the blood of the rabbit is $^1/_{151}$; in the dog $^1/_{153}$, $^1/_{203}$, $^1/_{194}$.

3. The fixation of urea in the tissues and blood does not seem to produce a diminution of muscular force.

4. The effects produced by the urea are not due to the presence of ammonia in the circulation.

5. When urea is injected into the blood, or when solutions of urea are used subcutaneously, the tissues take up great quantities of the urea.

6. The toxic doses in man are in every respect comparable to those in animals. Physiology and clinical facts reach the same conclusions; urea is a poison whose toxic dose can be determined with exactness.

MATERIA MEDICA AND THERAPEUTICS.

ERGOT IN THE TREATMENT OF CONSTIPATION.—Dr. Granzio (*Allgemeine Medicinische Central-Zeitung*, *Practitioner*) reports two cases of constipation following the abuse of purgatives, relieved by ergot. Three doses of 10 grains each were given at intervals of two hours, and were followed by a copious evacuation. A second stool occurred spontaneously the next day, and after the administration of ergot in smaller doses for a few days a definite cure was obtained. The constipation was due to atony of the muscular wall of the intestines.

THE

Journal of American Medical Association.

PUBLISHED WEEKLY.

THE EDITOR of this JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information, should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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CHICAGO, ILLINOIS

SATURDAY, JANUARY 17, 1885.

STETHOSCOPES.—In the first number of the current volume of the JOURNAL, at page 15, will be found an abstract of the views recently published by Haupt, in the *Aerztliches Intelligenzblate*, concerning the "Perception of Sound in Auscultation." The conclusion at which he arrives is, that the sounds are transmitted through the solid part of the stethoscope, and that the hollow part or tube is entirely superfluous. Knowing that the question presented by Haupt had been thoroughly investigated, and the comparative value of solid and hollow or tubular stethoscopes fully tested clinically in this country many years since, we let the abstract appear solely for the purpose of re-calling attention to the subject, which is one of much clinical importance. Even sooner than we expected the statements of Haupt have elicited the three following letters in reply, which so fully negative the views of Haupt, that we will give them instead of further comments of our own; only adding that we think we recognize in the letter signed "B" the words of one of the most experienced and eminent members of the profession in the New England States. We also fully agree with his expression in the P. S. to his letter, to the effect that Camman's binaural stethoscope just as he left it, is really the best instrument for auscultatory purposes that we have.

DENVER, COLORADO, Jan. 8, 1885.

TO THE EDITOR OF THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION:

Dear Sir:—I have just been reading your resumé, in the Jan. 3d number, of an article by Haupt in the "*Aerztliches Intelligenzblate*," entitled "Perception of Sound in Auscultation." Allow me to ask: Did Haupt present any solid "hearing staff" or an instrument built on the *solid* plan to substantiate his conclusion that "the bore of the stethoscope is quite superfluous"?

It is important to know if Haupt's conclusions are theoretical merely, in which case they are of doubtful value, or, if obtained by a comparison of instruments, with what imperfect stethoscope his solid instrument was compared.

If I mistake not, there is something wrong in the gentleman's premises. Most likely it is in the statement that the aid of the stethoscope "as a hollow resonator is lost, *as we use the instrument*, for the tube no longer acts as a resonator when we block up both ends of it." The fault lies in not appreciating that in a perfect instrument as to the transmission of sound, with large-sized tubes of gradually decreasing caliber from the bell to the ears, and with smooth, even, inner surfaces, the column of the contained air has imparted to it an impression of the transmitted sounds, which impression is carried on to the tympanum and thus serves to correct the sound given by the solid parts of the instrument. With a suitable stethoscope this matter can be easily tested. By putting cotton in one's ears, or by stopping up either or both ends of the binaural stethoscope, and thus making it more like a solid instrument, the dulling of the perception of the transmitted sounds can be appreciated. I speak of a *suitable* instrument, because most of the cheap instruments now in the market are not so. Such an one should combine the good qualities mentioned above, with such shaped bells as have broad impinging surfaces where they are pressed against the chest walls, and the coiled wire of the flexible portion imbedded in soft rubber, giving a smooth inner surface. Messrs. Chas. Truax & Co., of Chicago, had the one made for me which I now have. It has all the improvements here recommended, and is a very satisfactory instrument. I doubt if it can be equalled in the clear transmission of intra-thoracic sounds by any solid "hearing staff." Hence these interrogatories. Very respectfully,

CHARLES DENISON.

EDITOR OF THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION.

My Dear Doctor:—In the issue of the 3rd instant you give a quotation from the *Medical Chronicle*, which gives to Haupt (*Aerztliches Intelligenzblate*) the credit of having shown that the vibrations started by the heart or lungs are not communicated to the ear through the column of air in the tubular stethoscope, but through the solid material of which the instrument is constructed.

It will be a matter of interest to your readers to know that an ingenious physician of this city showed the same thing to the Chicago Medical Society about four years ago, but that he soon found that he was mistaken.

I was not at the meeting when the discussion took place, but the next day the doctor called at my office and demonstrated to me the interesting fact that a watch placed against the chest piece of a stethoscope

could be heard quite as well with the tubes of the instrument completely closed as when they were free. He therefore concluded that the vibrations were transmitted through the material of which the instrument was made, and he reasoned that if the stethoscope were made of a solid piece of wood the respiratory sound would be transmitted to the ear with more intensity than through the tubular instrument.

I suggested that we try the experiment over the trachea, from which the most intense respiratory sounds are to be obtained. We did so, and found that the tracheal sounds could not be heard at all when the tubes were plugged. I have often used the solid stethoscope in auscultatory percussion, but I have never been able to hear the respiratory sounds through it.

Theoretically, the solid stethoscope is all that Haupt claims for it, but practically it is worthless, and consequently we conclude that something is wrong with the theory. Any one who will take the trouble to plug the tubes of his stethoscope, and then attempt auscultation of the lungs, may speedily convince himself that Haupt is not correct in his conclusions. E. FLETCHER INGALS.

64 State street, Jan. 10, 1885.

BOSTON, Jan. 8, 1885.

DR. DAVIS:

Dear Sir:—In the last numbers of the JOURNAL there is an extract (page 15), on "Perception of Sound in Auscultation," by Haupt. The writer theorizes a great deal, and his final conclusion is "that the bore of the stethoscope is quite superfluous, and a solid 'hearing staff' with a wide ear piece and a wide chest piece is the theoretically perfect instrument."

I *theorized* about thirty years since in the same manner; but *facts* upset my theory. My three *solid stethoscopes* remain now as relics of past experiments that were fruitless. Soon after these futile attempts on *theoretical grounds*, that skillful auscultator, Dr. C. P. Camman, of New York, kindly sent to me one of his Binaural Stethoscopes which compelled me to give up *all single* stethoscopes, whether perforated or not.* This admirable instrument *proves* that the position theoretically taken by Haupt (even the "vibrations started by the heart and lungs are incompetent to impart audible vibrations to a light medium like air,") is wholly unfounded in facts. Probably Haupt never saw a Camman stethoscope, or he would never have advanced such opinions.

Yours truly, B.

COLLECTIVE INVESTIGATION OF DISEASE.—At the last annual meeting of the Pennsylvania State Medical Society, a committee was appointed on this subject, consisting of Drs. James Tyson, Charles R. Mills, and Robert N. Chase. A letter from the chairman of the committee, Dr. James Tyson, states that they have prepared a circular and cards on which to record observations for acute pneumonia, chorea, and acute rheumatism, and endeavored to place them in the hands of every member of that Society. If any member has been accidentally omitted, he can obtain copies by applying to the chairman of the committee, 1506 Spruce street, Philadelphia, Pa.

The cards and questions and diseases are the same as used by the committee of the British Medical Association, for the purpose of making the returns capable of comparison in both countries. The parties receiving these cards are requested to fill and return them to the Chairman of the Committee before the first day of April, 1885.

At the annual meeting of the Illinois State Medical Society, held in May last, a committee was appointed on the same subject, consisting of Drs. N. S. Davis, J. H. Hollister, J. F. Todd, E. P. Cook, and G. Wheeler Jones. This committee had printed, memoranda and cards in relation to the same diseases mentioned above and some others, and through the Secretary of the Committee, Dr. J. F. Todd, of Chicago, they were distributed to a large part of the members of the Society more than three months since. How many have been filled and returned to the Secretary we have not learned. But it is very desirable that all should be returned to the Secretary of the Committee before the first of April next. We think a committee on the same subject was appointed by the Ohio State Medical Society, and perhaps by some others. As this method of investigation has now become international, we would feel greatly obliged for information directly from all existing committees of State Societies in this country, that we may be prepared to report the exact degree of progress thus far made, to the next meeting of the American Medical Association, and also to the Secretary-General of the International Collective Investigation Committee in London.

ANNALS OF SURGERY.—We have just received the first number of this new periodical, published simultaneously in the United States and Great Britain, and edited by L. S. Pilcher, M.D., Brooklyn, N. Y., and C. B. Keetley, F.R.C.S., London, England. The number contains 96 pages on good paper, and published in good style by J. H. Chambers & Co., of St. Louis, Mo., at $5 per annum. Its pages are devoted exclusively to surgical science and practice; and the reputation of the editors is sufficient assurance that it will be worthy of the patronage of all interested in the practice of that department of medicine.

*Of late years, the manufacturers of the Camman double stethoscope divide it into two parts and fail to give to it the proper curves laid down by Camman. I deem this unfortunate for the profession, although it is still far superior to any *single* stethoscope.

OMISSIONS.—In the list of permanent members of the American Medical Association, published in the JOURNAL of December 27, 1884, the names of Warren W. Foster, M.D., Putnam, Conn., and P. C. Williams, M.D., Baltimore, Md., were accidentally omitted. In the same list the residence of Dr. G. B. Ward should be Fairbank, *Iowa*, instead of Ohio.

SOCIETY PROCEEDINGS.

SUFFOLK DISTRICT MEDICAL SOCIETY.

Albert N. Blodgett, M.D., Secretary.

SECTION FOR CLINICAL MEDICINE, PATHOLOGY, AND HYGIENE.

BOSTON, MASS., Dec. 10, 1884.

The meeting was called to order at 8 o'clock, by Dr. R. T. Edes, P.M., chairman.

Dr. H. I. Bowditch asked the privilege of addressing the Section for a few minutes, prior to the transaction of the business of the evening, and spoke as follows:

Before we enter upon the regular business of this Section, I beg permission to announce the death of one of the ablest of the members of the Massachusetts Medical Society. I allude to Henry A. Martin. He was, in my estimation, one of the strongest of the medical men it has been my fortune to meet either here or elsewhere. His long residence in Roxbury, and his frequent intercourse with the physicians and surgeons of Suffolk and Norfolk counties, seem naturally to call upon us, who as a Section of the State Society meet first after his death, to take some notice of that event, and of the loss which the profession has sustained.

Dr. Martin died of diabetes, quietly, and apparently without suffering, during the forenoon of December 7, 1884. He had been fully aware for two years that the disease had fastened upon him, but he made little or no complaint, even to his family, and kept steadily at professional work until within a few past weeks. Finding that his strength was gradually but surely lessening, he had thought of resigning all laborious out-of-door practice and attending only to consultations at his office. During the last weeks especially, his physical debility was greatly increased, and he kept his bed, but with no improvement.

Believing that death was near at hand, he calmly bade farewell to his family, and soon after fell into a quiet sleep, from which he never awoke. Let us hope that such a tranquil ending of this life may be vouchsafed to us all.

In looking upon Dr. Martin's life and character, we must, I think, admit that in most of his salient points he was a man "*sui generis*." I at least know of no one like him. Intellectually he was far above most persons I have met, and physically he was one of the tallest and seemingly one of the most stalwart.

In conversation, not only upon medical subjects, but upon a large range of topies, some of them widely removed from professional thought, he was superior to the majority of men. He had studied deeply into the history of medicine. In this department I know but one of our living associates who could approach him. He gloried in the greater heroes among the medical men of the past. Especially was he drawn to any one who by native vigor of intellect, and perhaps fierce professional contests, had won for himself fame in spite of all the babbling opposition of men who apparently hated him because of his very superiority to themselves. For example, he sympathized heartily with that giant of our profession, grand old Ambrose Paré, who gained his ever-enduring fame in spite of the sneers of the robed professors of the School of Medicine at Paris, and the carping criticisms of smaller men in the profession at large. From a similar superiority of mind, and perhaps we may allow, from an apt readiness and joy in intellectual controversy, and from the fact that he never concealed an opinion though it struck rather severely, or perhaps sarcastically at even a friend's favorite notion, some have been led to think that Dr. Martin was too pugnacious. Truth compels me to admit that at times he may have been almost severely hypercritical in allusion to the writings or actions of others. Especially was this trait manifested whenever or wherever he knew there was a lack of fair dealing on the part of an opponent. But he was always a brave and frank-speaking antagonist. I have had some sharp passes with him at times, but our friendship was never marred by them. Nay more—I can say truly that my respect for him has always been rather increased than lessened by these contests, for I found him always open and fair in his opposition to my views. Anything like cant or hypocrisy or unfair dealing, exhibited by an opponent, seemed to awaken in him a sort of fury, and he unsparingly used the lash upon his antagonist. With a kind of Martin Luther violence he would at such times hurl defiance at anything or person that arose before him, and right manfully he would defend the position he had taken. I grieve to say that, in consequence of this peculiarity, some of the more sensitive of those whom he felt himself called upon to criticise may not hold his memory in their hearts so gratefully or so inspiringly as I do. I shall always regret his loss, because our relations were always friendly, notwithstanding an occasional difference in opinion, and from him I could often gain excellent information which I could get from none else.

Before closing these remarks, which are so much of a merely personal character, I would refer to the fact that, in addition to having been an able medical practitioner amongst us, he has been of inestimable advantage to every community in the world where animal vaccination is now used. His energy and decision of character, his knowledge of the past, have brought us all back to first principles on vaccination, as proposed by Jenner.

One at least of his surgical devices is almost uni-

versally used, not only in this country, but in Europe, and probably elsewhere where surgery is practiced.

For the above reasons and because he was an able practitioner of medicine and surgery for many years amongst us, and because medicine has been helped onward by his life's work, I have wished that due honor should be paid to the memory of our dead associate.

Believe me, gentlemen, you will wait for a long time before a man comparable with him will arise amongst you.

By motion of Dr. Blodgett, seconded by Dr. J. P. Reynolds, Dr. Bowditch was requested to convey to the family of Dr. Martin the regrets of the Section at the loss of one of the distinguished members of the medical profession.

SACCHARATED OXIDE OF IRON.

Dr. G. L. Walton exhibited a preparation of iron to which he had alluded some months ago in a published article, but which had not become generally known to the profession. He said:

It may possibly be remembered that I called attention [1] some time since to several agreeable iron preparations in common use in Germany, but not as yet introduced into America. The merits of one of these preparations especially impressed me, namely, the so-called *Eisenzucker* (*Ferrum Oxydatum Saccharatum Solubile*), and I am glad to be able to show a specimen of it this evening, and to state that Metcalf & Company have undertaken its preparation, with this very satisfactory result. It is a brownish-red powder, of a very agreeable, sweet taste, completely soluble in five parts of water, and containing three per cent. of metallic iron. It does not discolor the teeth, and on account of its ready solubility may be taken for an indefinite length of time without affecting the digestion, even in cases where iron is otherwise ill borne. It will be found especially valuable in chlorosis, for susceptible nervous patients and for children. The dose is five to twenty grains three times a day, to be taken in water or in powder. Most convenient and agreeable will be found the tablets, of which I have here five and ten grain specimens. Mr. Davidson assures me that this particular sample contains three and one-half per cent. of iron by actual assay. He also tells me, it may not be out of place to state, that he finds the precautions given me by the Leipzig pharmacist, Herr Blaser, of the utmost importance, and that a much more satisfactory result is reached by following his directions (detailed in the article already alluded to) than by following simply those of the pharmacopœia. The difficulty of making the preparation may have had something to do with the postponement of its introduction in this country. Its value, however, once recognized, I feel sure it will be generally adopted, and it is rather remarkable that among the needlessly multiplied and unsatisfactory forms of iron in American pharmacy this really elegant preparation has been so long overlooked. To show that it is not identical with, and that it is far superior to, the saccharated carbonate of iron, I have brought a specimen of the latter, which has very much the color and general appearance of dirt, and is not particularly agreeable to the taste. I pass around two bottles containing about five grains each of the powders and half an ounce of water. It will be seen that, while the *Eisenzucker* makes a perfectly clear red solution, the carbonate is only partially dissolved, giving a murky mixture with a black residue. (Dr. Edes asked for the composition, saying that it could hardly be an oxide if soluble in water.) In answer to Dr. Edes: I cannot find that the chemical formula has been made out. It is certainly more than doubtful if it is really an oxide, notwithstanding its name. It is mentioned in the United states Dispensatory under *Ferri Peroxidum Hydratum* as a *so-called* oxide of iron adopted in the German pharmacopœia. The process of manufacture was then described briefly.

In answer to Dr. Webber, Dr. Walton said that he did not know how long the solution in water remained unchanged, but that it did so for several days he could state with certainty from observation.

DR. J. J. PUTNAM then presented the first paper of the evening, entitled

A STUDY OF THE DEVELOPMENTAL DISEASES OF THE NERVOUS SYSTEM,

which is reserved for publication.

Dr. P. C. Knapp presented a very complete and exhaustive study of

CUTANEOUS AND DEEP REFLEXES,

extending over a long time and comprising several hundred observations, an abstract of which is here given:—

The observations were made upon 239 patients in the nervous and renal service at the Boston City Hospital.

The frequency of absence of the cutaneous reflexes was as follows.

	Cases Examined.	Present.	Absent.
Plantar	234	217	17
Cremaster	167	146	21
Gluteal	179	113	66
Abdominal	239	142	97
Epigastric	239	97	142
Erector spinæ	178	45	133
Scapular	177	15	162

In nearly every case in which the plantar and cremaster reflexes were absent there was either some lesion of the reflex arc, as in neuritis or myelitis, or some cerebral disturbance like coma, hemiplegia, or convulsions. No pathological cause could be assigned for the absence of the other reflexes. When the reflexes differed on the two sides it was usually significant of some unilateral disturbance of the nervous system, but in one or two cases no such disturbance could be made out.

The frequency of absence of the deep reflexes was as follows:—

[1] Boston Med. and Surg. Journal, January 12, 1882.

	Cases Examined.	Present.	Absent.
Patellar	239	192	47
Tibial	231	18	213
Ankle clonus	238	12	226
Toe clonus	239	0	239
Triceps	239	198	41
Radial	239	131	108
Ulnar	239	61	178
Extensors of wrist	239	25	214
Wrist clonus	239	0	239
Costal	234	161	73
Lumbar fascia	178	33	145
Spine of scapula	178	45	133

The patellar reflex was also tested in eighteen patients under ether, and was found absent in six.

The patellar reflex was found absent not only in cases of myelitis, locomotor ataxia, multiple neuritis, etc., where there was evidence of lesion of the reflex arc, but also in many cases of cerebral disturbance, like coma, cerebral hæmorrhage, and obscure cerebral disease, especially in alcoholic subjects. The theory was advanced that such disturbances in the brain might have an inhibitory action upon the reflex centres in the cord, abolishing their action.

The tibial reflex was found usually in cases where the patellar reflex was exaggerated, but was not necessarily pathological.

Ankle clonus was found four times immediately after convulsions; in other cases there was disease of the lateral columns. Its presence was regarded as always pathological.

In a few cases of exaggerated patellar reflex, patellar clonus was tested, and was found in five cases, in four of which there was disease of the lateral columns. It was regarded as pathological.

The presence of the reflexes of the upper extremity was found to be very common. The cause of absence was not clear.

The costal cartilage reflex was found very often, and no clear relation between its presence and the existence of phthisis was apparent. The other reflexes were unimportant.

A difference of the deep reflexes on the two sides was usually significant of unilateral nervous disturbance, but as with the cutaneous reflexes, one or two cases were found where no such disturbance could be made out.

Dr. Webber said that it gave him pleasure to testify to the correctness of the observations, as they were made when Dr. Knapp was house officer, during Dr. Webber's term of service at the City Hospital. He was able to "control" many of Dr. Knapp's experiments, and was satisfied that they were thoroughly reliable. They constitute a valuable addition to our recorded knowledge upon the character or various reflexes under a great variety of conditions.

TENDON REFLEX AFTER FRACTURE OF THE PATELLA.

Dr. Walton also introduced a cast of a knee, which he felt sure would be of interest, not only from the rarity of the case, surgically considered, but also in connection with Dr. Knapp's paper. The cast was obtained (through the kindness of Dr. W. J. Otis) from a patient who was under treatment in the nervous Out-Patient Department of the Massachusetts General Hospital for another trouble, but who had an ununited fracture of the patella, with separation of the fragments measuring six inches when the knee was flexed, three and a quarter inches when extended. The only difficulty experienced in walking, by the way, was in going up and down stairs. The interesting point in connection with the subject was that while no reflex could be obtained by striking below the knee, a distinct reflex followed the tap just above the upper fragment, the muscle being felt to contract, the patella fragment to be drawn up, and the skin over the joint to retract, although, of course, no motion of the foot followed.

Dr. Edes spoke of the varying results which will be obtained according as the blow is struck with a greater or less amount of force, and penetrates more or less deeply into the tissues. The study of the phenomena of the patella is very interesting. This is entirely absent only in those cases in which the nervous centers may be supposed to be diseased.

Dr. Putnam moved that the report upon the recent epidemic of cholera in Paris, which the secretary had been requested to prepare, be postponed to the next meeting. Adjourned at ten o'clock.

DOMESTIC CORRESPONDENCE.

DEAD TEETH IN THE JAWS.

To the Editor of The Journal of the American Medical Association.—*Sir:* *The Medical Record* of November 8th contains a reply from Dr. Samuel Sexton to gentlemen who have criticised, in the columns of that journal, his previous article on "Dead Teeth in the Jaws"; and I beg space to present some views on this long neglected, but very important subject of lesions of the dental organs.

If, by means of this discussion, members of the profession are brought to more fully realize the value of faithfully bestowing upon the teeth the care essential to their preservation—of adopting proper hygienic measures to this end, and of appreciating the difference between the scientific treatment of dental diseases and the treatment too often resorted to by uneducated dentists, the laity as well as the profession at large will be greatly benefited thereby.

Dr. Sexton says: "The retention in the jaws of teeth which are diseased, from death of the pulp or from caries, have become irremediably sensitive to thermal influences, or have been deprived of adequate periosteal nourishment through calcareous formations about the roots, very frequently gives rise to nervous diseases about the head. I am convinced that these reflected nerve influences manifest themselves much oftener since dentistry has come more extensively into practice during the present generation, and greater efforts are made to retain defective teeth in the jaws."

That diseases of the teeth are often the center from

which pain is reflected to the eyes, ears and other parts, all experienced clinical observers must admit. But that these pathological conditions of the teeth, from which reflected pain has its origin, can be and are successfully treated and cured with rare exceptions, as effectually as any other diseases, is a fact too well established to be set aside.

It is not possible to describe in this letter the methods by which the various diseases of the teeth are treated, but suffice it to say that "teeth which are diseased from death of the pulp or from caries" *do not* "become irremediably sensitive to thermal influences." In proof of this statement, many thoroughly educated medical men, practicing the specialty of dental surgery, will testify.

"Teeth deprived of adequate periosteal nourishment, through calcareous formations about the roots, very frequently give rise to nervous diseases about the head." To this statement I assent, but dissent as to the remedy not mentioned but implied, *i. e.*, the removal of the teeth. If the calcareous deposits mentioned have destroyed so much of the pericementum together with the alveolar processes as to render the teeth very loose; if, indeed, the teeth have lost their bony support and are retained by means of a remnant of pericementum only, they can not, of course, be restored to permanent health and usefulness, and their removal is, therefore, indicated. Teeth in this condition "frequently give rise to nervous diseases about the head."

On the contrary, if the calcareous deposits have not destroyed the pericementum and alveolar processes to a very great extent, the condition is amenable to successful treatment and cure. In answer to the assertion that "Reflected nerve influences manifest themselves much oftener since dentistry has come more extensively into practice during the present generation," I would say, that with equal propriety it might be said that reflected nerve influences manifest themselves more frequently since gynæcology has come more extensively into practice. To attribute the obvious increase of nervous diseases during the present generation to diseases of the teeth is a statement not only "sweeping," but "overdrawn." *Much harm* is no doubt done by some of the modern appliances "for retention in the mouth of substitutes for absent teeth," and the unhealthy state of the gums and contiguous parts, established and maintained by the presence of these substitutes, unquestionably give rise in many cases to reflected pain.

When Dr. Sexton attempts to establish a *law* governing the management of diseased teeth, it must be based upon more substantial grounds than those which he presents. The case related of his patient, the "medical man, who practices dentistry," and who was convinced that an inflammation of one of his ears began from the time the upper second molar of that side was treated for a diseased pulp, is simply an assumption, on the part of the patient, that the ear trouble had its origin from the diseased tooth, and the patient's diagnosis of his own case seems to have been accepted by Dr. S. as conclusive. The ear disease in this case may have emanated from the diseased tooth, but no evidence is produced to that effect. In regard to the query as to "whether it is safe practice to retain dead teeth in the jaws," I would say that thousands of people in our own country have had pulpless (not dead) teeth in their jaws many years, which are exempt from pericemental disease, and which serve all the purposes for which teeth were provided. To ask whether it is safe practice to retain these, so-called, dead teeth in the jaws when they have been comfortable and useful from ten to forty years and promise to remain so through life, seems like a proposition too injudicious to need comment. While the death of the pulp results in "cutting off the source of nutrition from the dentine," it does not follow "that in a large number of instances irritation can not be easily controlled."

Neither does the tooth become a foreign substance. The dentine and the enamel are, of course, no longer nourished after the death of the pulp, but their resisting structure renders them capable of maintaining their integrity many years after the pulp has been removed; and the pericementum will nourish the cementum and thereby retain the tooth in its alveolus in a comfortable condition. In order, however, to thus retain the tooth and prevent inflammation from supervening, the devitalized pulp must be removed, the pulp canals thoroughly disinfected and filled with a plastic material which hardens when in position. Dr. S. most clearly exhibits his imperfect knowledge of the dental operations in vogue, when he says: "Inflammation of exposed dentine cannot surely be entirely arrested in any case by filling the pulp cavity with any known extraneous material, and especially is handicraft wanting to even imperfectly protect the minute and often tortuous canals leading down to the apical foramina of the majority of the teeth." To arrest "inflammation of exposed dentine by filling the pulp cavity," in the opinion of Dr. S. would seem to be most desirable. How a tissue without nourishment and consequently without vitality can take on or maintain inflammation is beyond comprehension. The impervious filling which I have herein mentioned will close the apical foramina, together with the canal, which "in the majority of cases" *is not* tortuous to a degree of rendering the perfect filling of the root difficult or uncertain, and the assertion that the dental surgeon "is able only to offer a hopeful but uncertain prognosis in these cases" is contrary to well established fact. There are no diseases to which mankind is heir more scientifically and effectually cured than the diseases of the teeth in question.

Again: "The dead tissues of the dentine will sooner or later, most likely, be transmitted through the tissues of the cementum to the periosteum." Communication between the lacunæ and canaliculi of the cementum with the tubuli of the dentine is not free; indeed, it seldom exists, hence it cannot be "that through the periosteum alone the dentine may long derive some nourishment."

About 22,000,000 of teeth are annually extracted in the United States, and I regret to say that this enormous loss of teeth is to no small extent due to the indifference manifested by physicians in the anat-

omy, physiology and pathology of these organs. It is a fact that no one will attempt to gainsay, that hygienic measures directed toward the preservation of the deciduous set, if understood, are seldom recommended by the general practitioner to the families under his charge. The premature loss of these teeth paves the way for early lesions of the permanent set. The pain resulting from advanced caries of the deciduous teeth, owing to the difficulties encountered in controlling the patient, is not easily treated; moreover, the injurious impressions thus made upon the system of the child abide through life. There is no doubt that hundreds of thousands of teeth are unnecessarily extracted each year, and then drugs are given with a view of curing the patient of the disorders of digestion and other abnormal conditions which follow, and which in turn arise from imperfect mastication of food, verily for the want of teeth.

We need to know "what's the matter" in the treatment of these "nervous diseases about the head," as in all others, and apply a remedy which will bring the abnormal tissues back to health. Too often, indeed, has it happened that patients, by advice of their medical attendants, have submitted to a loss of many, and in some instances all their teeth, in the vain endeavor to get relief from trigeminal neuralgia. You may ask, Why this useless loss of teeth, and all the evils resulting therefrom? Because the advice given was not wise; the etiology of the affection was not understood.

There are certain pathological conditions of the teeth which have not been mentioned in this discussion, and which give rise to reflected pain to the eyes, ears and other parts.

Exostosis of the fangs of teeth and nodules of calcific matter within the pulp canals in contact with a living pulp. The former of these conditions has been regarded incurable, the removal of the tooth with the united bony tumor being indicated. In favorable cases, however, this tumor may be excised and removed without removing the tooth. The pulp nodules or calcified deposits within the pulp chamber may be, in a large majority of cases, successfully removed without sacrificing the tooth.

No one approves more than I the removal of the causes of disease. It is no more necessary to extract a tooth at the root of which an alveolar abscess has formed than it would be to amputate a limb for the cure of an abscess of the medullary substance of its bone. Disease of the eye sometimes requires that it be enucleated, but the honest, skilled ophthalmologist *would not* remove the eye when he *knew* he could restore it to usefulness. The spirit of the teachings of Dr. Sexton's articles is far from being progressive. Nor is this all; many assertions are not based upon fact, but upon erroneous impressions. Our duty to our profession and the laity is not to destroy, but to save; and while ignorance is ever working its mischief in all vocations in life, it is not just to accept the results of such work as a basis upon which to found a law.

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NEW YORK LETTER.

NEW YORK, Dec. 20, 1884.

For some time past the subject of cholera has received a good deal of attention in the societies here, and it may, perhaps, be of interest to note the views of some of the prominent medical men of New York in regard to Koch's theory of the disease so far as they have been expressed at these meetings. The one who has expressed most fully and forcibly his belief in the probable correctness of the opinions of Koch is Dr. Austin Flint, Sr. That Dr. Flint would be likely to take this position whenever he enunciated his convictions on the subject was rendered almost certain by the elaborate paper which he read in support of the essential dependence of tuberculous disease on the *bacillus tuberculosis* at the first public meeting of the New York County Medical Association in January last. At the October meeting of the same society he read an equally elaborate paper on "The Parasitic Doctrine of Epidemic Cholera," in which he maintained that in order to form an opinion as to whether the disease was due to a specific micro-organism, it was not necessary to have personally verified the evidence which the microscope afforded; but that this claim, like many in courts of law, was to be substantiated or rejected on the testimony of a sufficient number of competent judges, such as alone could arrive at any sound conclusions. It was logically certain that the diseases now classed as infectious diseases involved each a distinct specific causative agent of some kind, and that this agent was a living organism was the most rational supposition. It was, therefore, noteworthy that the discovery of micro-organisms as causes of infectious diseases, so far from being in opposition to previous views, was, in reality, a confirmation of the prevailing views of the ætiology of these affections. At the present time there was no room for doubt as to the dependence of certain infectious diseases on specific micro-organisms, and if it were demonstrated (as had indeed been done in the case of several) that any single one of these diseases was caused by a specific parasitic organism, it certainly seemed to be a rational inference that they all have a similar causation. In regard to Koch's researches in epidemic cholera, he said that if it were established that the comma bacillus was invariably present in the disease, and that this parasite was never present except when the cholera existed, as claimed, it would be logically certain that there was an essential pathological connection between this parasite and the disease; but the crucial test of inoculation would be required for determining a causative relation between the two. In tuberculosis the proof that the parasite peculiar to that affection, after a series of cultivations, was capable of producing the disease by inoculation, was absolute; but up to that time Koch had reported failure in his efforts to obtain similar proof in the case of cholera. A few days before Dr. Flint read his paper, however, there was first published in this country an account of the experiments of Rietsch and Nicati at Marseilles, in which they claimed to have succeeded in producing cholera in the case of

dogs, guinea-pigs and other animals by the introduction of a pure culture of comma bacilli into the duodenum, after ligation of the ductus choledochus. Still, failure to inoculate would by no means disprove the parasitic doctrine of cholera, since it had been shown that all animals were not susceptible to the morbific action of micro-organisms which were known to be the causative agents of specific diseases in man. Aside from this positive proof, there were certain ascertained facts which aim to sustain the parasitic doctrine of cholera. If it were accepted that, reasoning by analogy, all infectious diseases are regarded as parasitic, the pathogenic claims of the comma bacillus over other intestinal bacteria seemed, with our present knowledge, to be paramount. There are certain well-marked lesions of the mucous membrane of the small intestine which are either due to the presence of this parasite, or they furnished a peculiar soil for its cultivation; in the latter point of view the parasite being a product of the lesions. Now, epidemic cholera, in all parts of the world except India, was an exotic disease and of rare occurrence, and it seemed, therefore, vastly more improbable that the lesions precede the presence of the parasite than that the latter exists prior to, and is the essential cause of, the lesions. Koch's observations had shown that the comma bacillus was very short-lived, and they were in harmony with the observations made in cases of cholera and with the clinical history of the disease. Cholera was a disease remarkable on account of the brief duration of its career, which was not longer than that of the successive generations of the comma bacillus when cultivated outside of the body. Again, some striking facts had been observed by Koch in India which went to show that the parasite had been introduced into the body prior to the development of cholera. Such is a synopsis of the argument made use of by Prof. Flint. In summing up, he said that to accept a doctrine prematurely because it was new and attractive would not be less illogical than an irrational skepticism; but while a proper sentiment of conservatism should lead us not to commit our minds unreservedly until sufficient data had accumulated to serve as a solid basis for a judicial decision, he believed it altogether probable that the decision would be in favor of the doctrines named. In the discussion which followed, Dr. E. G. Janeway expressed himself as in entire accord with the views of Dr. Flint in regard to the parasitic origin of the disease.

At a meeting of the New York Academy of Medicine, held in November, Dr. Frank H. Hamilton read a paper giving an account of the outbreak of cholera at Suspension Bridge in 1854, and also presenting his personal views in regard to our present knowledge of cholera. In the first place, he said, we had no positive knowledge of the existence of a specific cholera germ, though it seemed highly probable that there really was such a germ. In regard to the comma bacillus of Koch, he said that H. Van Dyke Carter, of Bombay, had proved that this microbe was not infrequently wanting in cases of cholera, and that it was occasionally found in diarrhæal diseases other than cholera. Again, he said, Surgeon-Major Timothy Lewis, of the British Army Medical School (Netly), had found that comma-like bacilli, identical in size, form, and in their reaction with aniline drugs, with those found in choleraic dejecta, were ordinarily found in the mouths of perfectly healthy persons. (Dr. Flint had also alluded to Mr. Lewis's claim, and said in reference to it that it did not appear that the culture-test of the faucial bacilli had been resorted to, while if the statement made by him was incorrect it could not fail to be disproved by other microscopical observers.) Finally, Finkler and Prior had found the comma bacillus in cholera morbus. He then referred to the experiments of Rietsch and Nicati, and said he thought their conclusions should be received with hesitation, since, so far as he was aware, none of the kinds of animals claimed to have been inoculated with cholera have ever been known to contract cholera through natural sources; and it seemed to him probable that the surgical procedure adopted by these experimenters had excited an irritation in the intestinal tract which gave rise to symptoms resembling those of cholera. Moreover, one of the French cholera commissions, while failing to inoculate animals by means of the comma bacillus, had succeeded in doing this by injecting into them the blood of cholera patients. On the whole, therefore, it seemed to him that Koch's claims for the comma bacillus had not as yet been substantiated. If, as seemed to be a fact, he went on to say, there are several microbes found in the intestines and dejecta of cholera patients, we did not know which one of these might be the active causative agent, or whether the cholera infection was not conveyed by the fluids in which they were found, rather than by the microbes themselves. The theory of Koch, that the infection entered the system through the alimentary tract alone was not proven, and he believed there was good reason for supposing that it also entered by means of the respiratory organs and was diffused through the circulatory system.

In the discussion which followed, Dr. Francis A. Delafield said he thought that at the present day all would be willing to acknowledge that cholera belonged to the class of diseases believed to be due to the agency of some specific poison. The latest theory in regard to the cholera poison or germ was in accordance with the prevalent opinion of the times. There are always fashions in the scientific world, which are sometimes good and sometimes bad, and the present fashion was to refer the causation of all infectious diseases to particular minute living organisms. While Dr. Hamilton had stated the evidence for and against this hypothesis in the case of cholera, he had not stated (a matter that it was important to have in mind), that out of quite a number of experimenters who had investigated the subject, there might in reality be only one or two whose testimony could be accepted as worthy of credence. The *technique* of such investigations was exceedingly laborious and complicated, and he believed that there are extremely few individuals who are sufficiently expert in its details to be capable of making trustworthy observations. As to the proba-

ble correctness or erroneousness of Koch's conclusions, Dr. Delafield did not commit himself to any opinion.

Dr. Alfred L. Loomis thought that cholera belonged to that class of infectious diseases in which the special poison is developed in a living organism, but is in a passive state when it leaves such organism, it being necessary that the agent should undergo certain changes in connection with decomposing organic matter, after leaving the body, before it is capable of giving rise to infection. He believed in the existence of specific cholera germs, and that in a large percentage of cases it entered the system through the intestinal tract. It remained true, however, that this sometimes occurred also through the respiratory tract, and all that one could say positively was that it undoubtedly entered the body through the mucous membrane. Although all previous researches in regard to the specific germ of cholera are insignificant compared with those of Koch, he would not be willing to accept the conclusions of the latter until it could be proved beyond any question whatever that the comma bacillus was always present in cases of cholera, that it was never found in any other disease than cholera, that it was never found in the healthy body, and, finally, that inoculation with it would give rise to cholera.

At the November meeting of the County Medical Association, Dr. Charles A. Leale read a paper on the cholera epidemic of 1866, but no reference was made to the bacillous origin of the disease. At the meeting of the Association which was held December 15th, however, when there was a general discussion of the subject of cholera, Dr. Janeway spoke at length of the latest researches of Koch, which had so materially strengthened his position in the eyes of scientific men.

By disproving, in the most successful manner, the claims of Lewis, who thought he found the comma bacillus on the faucial mucous membrane of healthy individuals, and of Finkler and Prior, who claimed to have discovered it in cholera-morbus, and, on the other hand, by his own complete and satisfactory confirmation of the results obtained in inoculating animals by Rietsch and Nicati, Koch had now, he believed, added the "missing link" that was necessary to complete the proof, and had positively demonstrated the correctness of his views in regard to the parasitic doctrine of cholera. In one point, however, he differed from Koch. The latter thought that the infection always entered the system through the alimentary tract, but it seemed probable to him that it was sometimes received by means of the breath. It was possible, however, he thought, that it might be carried by the breath into the mouth, and then swallowed, as he believed was undoubtedly the case sometimes in typhoid fever. The President of the Association, Dr. Wm. Detmold, thought that while the bacillus was now the fashion, he was not satisfied that it had been proven to be the essence on which cholera depends. All new discoveries, he contended, had to be sifted through a great many cool brains before they could be accepted as scientific facts. Dr. Flint's remarks on this occasion were mainly of a practical nature, as Dr. Janeway had said all that he wished to add in regard to Koch's doctrines, and in case of a visitation of cholera he urged with great force the importance of a thorough house-to-house visitation on the part of the health authorities. The past history of the disease showed, beyond the shadow of a doubt, that the disease as a rule was thoroughly amenable to treatment in the early stage, and he believed that if every family in the community could be visited every day by a sanitary inspector, and have the inquiry put to it whether any of its members were affected with diarrhœa, cholera would be divested of a very large share of its fatality.

The Board of Estimate and Apportionment has added to the provisional estimate for the health department the sum of $50,000, to be used only in case the cholera comes; but when we reflect how vast would be the pecuniary loss to the city in case an epidemic should occur, it cannot fail to be seen how much better, from a monetary view, it would be that the appropriation should be used for additional sanitary work now, before the cholera comes, in order to put the city in the best possible condition in advance. The Board of Health is believed, by competent judges, not to be as efficient at present as it was under the administration of Prof. C. G. Chandler, and the reason for this is attributed to political influences. Thus, for months the manure dealers maintained a notorious nuisance; but no attempt was made to interfere with it by the board until the ladies of the neighboring district of Beekman Hill appealed to the grand jury and secured the indictment of the contractor, accompanied by a reprimand to the health board. In commenting upon the matter recently Prof. Chandler remarked: "I believe it was the action I took against those yards that lost me the presidency of the board, for the men connected with the manure business are rich and have a strong political backing." P. B. P.

WAVELAND, IND., Nov. 14, 1884.

DEAR JOURNAL:—The following extract from a letter, written by a young lady to her most intimate lady friend, gives an amusing account of a consultation held in her case at Lancaster, Pa.

Her physician Dr. A., being in doubt, takes her into the city to consult Dr. M., but the extract speaks for itself:

> Just imagine me seated in one of Mr. C's. parlors, with Dr. A. on one side and Dr. M. on the other, both questioning at the same time, "Put out your tongue; let me feel your pulse; please look me right in the eye, no harm to look the old doctor in the face. Does your back pain you much? Your side? Your head? Your breast? Allow me to hear the pulsations of your heart, no harm for the old doctor to place his ear to the young ladies' heart," etc., etc. You would not wonder if I could not keep from laughing or crying (I did not feel particular which), weak and sick as I felt myself to be.

I needn't add that they diagnosed the case and treated it successfully, but if my patients are going away to write me up in that manner I shall hereafter confine myself to essentials. By the way, I have lately had an interesting experience, as a large scar

on the thumb and another on the middle finger of my right hand remind me. On Aug. 1, 1884, I was called to attend Mrs. M. in confinement. She was a large, healthy, young woman, about to give birth to her second child. Two years previous to this she had gone through her first labor, and she told me she had a very tedious time of it and came near dying, as the labor was protracted for two days, and her suffering great all that time. A homœpathic physician, who attended her, failed to discover any cause for the delay.

I examined her and found the womb dilated the size of a silver dollar, and an absolutely dry labor. The walls of the vagina were dry and hot. I was not aware that I had an abrasion upon my hand until it came in contact with these membranes, when a place just back of my thumb nail and another one just over the first joint of middle finger on right hand began to smart and burn. The burning was so great that I went immediately and washed my hand, but that did not stop it. I could see two specks where the cuticle was off. Pursuing about the plan laid down by Dr. John Morris, page 522, Nov. 10, 1883, of THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION, I succeeded in delivering her of a ten-pound boy in about four hours. By that time there were two swellings over the infected points on my thumb and finger, about as large as, and had much the appearance of, half a ripe currant. I could not sleep that night, and was astonished next morning to see two large black tumors, about the size of half hazel nuts and looking like blood blisters. I opened them, but they would not empty. I applied soothing applications and took anodynes as the inflammation extended above my wrist, and the thumb and finger were badly swollen, very painful and sore. After losing three nights' rest, the sores sloughed out, leaving the skin near and around them violently inflamed, but the swelling of arm and hand began to subside, and I was comparatively free from pain, after which I made a favorable recovery. I am neither a drunkard nor a syphilitic, nor was the inflammation of an erysipalatous nature. I hallowed hello as I passed the house one day, and asked after the child, as I did not like to risk going in. The mother said it was all right, but had had very sore eyes. She had not had any trouble herself whatever.

I have practiced medicine and surgery twenty-three years, and am a great advocate of antisepsis, but had almost begun to wonder if there really was as much in it as Prof. Lister claims. If some medical brother, of more experience than I have with such cases, will tell me what the probable condition was in this case and will comment upon it, I will be much obliged to him. Yours, T. F. LEECH.

Jeff. Med. College.

MISCELLANEOUS.

OFFICIAL NOTICE.

ANNUAL DUES from members of the Association—*Five Dollars* per annum—are payable directly to the Treasurer. Such payment entitles them to receive the JOURNAL of the Association for one year.

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THE INDEX to the 33 Volumes of Transactions will be forwarded, postpaid, on receipt of *One Dollar* by the Treasurer.

According to a resolution passed May 9, 1884, at the Washington meeting, continuous payment of Annual Dues is required. to retain permanent membership.

RICHARD J. DUNGLISON, M.D.,
Treasurer.

Lock Box 1274, Philadelphia, Pa.

MISPLACED KIDNEY.

In view of the increasing frequency of operations upon the kidney, I deem the following case worthy of record:

The deceased, Wm. Snyder, at eleven years, came to his death by injuries received from a fall from a tree, and presented no symptoms of kidney trouble during life.

Post mortem examination revealed the left kidney normal in size, position and structure.

The right kidney was located on the lateral aspect of the bodies of the *last two lumbar vertebræ*, and received its blood supply from a branch given off from the *right common iliac one-half inch below the division of the aorta.* Nothing abormal in its size or structure was discovered.

The case occurred in the practice of Dr. A. E. Vanbuskirk, by whom the post mortem was made, with the assistance of the writer.

MILES F. PORTER, A.M., M.D.,
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OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, UNITED STATES ARMY, FROM JANUARY 3, 1885, TO JANUARY 9, 1885.

Shannon, Wm. C., Captain and Assistant Surgeon, relieved from duty at Ft. Bridger, Wy. T., and assigned as Attending Surgeon at Headquarters Department of the Platte. (S. O. 2 Department of the Platte, Jan. 5, 1885.)

Robinson, S. Q., Captain and Assistant Surgeon, assigned to temporary duty at Portland, Oregon, from Dec. 17, 1884. (S. O. 206 Department of Colorado, Dec. 22, 1884.)

Appel, A. H., Captain and Assistant Surgeon, granted leave of absence for one month, to take effect on or about Jan. 7, 1885. (Madison Barracks, N. Y.) (S. O. 268 Department East, Dec. 31, 1884.)

Wales, P. G., First Lieutenant and Assistant Surgeon, relieved from duty at Vansound Barracks, W. T., and ordered to return to his proper station, Ft. Coeur d'Alene, Idaho. (S. O. 204 Department of Colorado, Dec. 19, 1884.)

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE UNITED STATES NAVY, DURING THE WEEK ENDING JANUARY 10, 1885.

Ross, J. W., Surgeon. Detached from the "Monongahela," for treatment at the Naval Hospital, New York.

Wells, Howard, Passed Assistant Surgeon. Detached from Naval Hospital, Philadelphia, Pa., to the "Monongahela."